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TEMPERAMENT: A SURVEY OF ANIMAL STUDIES

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Temperament is the raw stuff of individuality. Temperament accounts for the original uniqueness of each organism. It is that part of one which is independent of the conditioning effects of the physical environment and the social culture. When temperament is refined by these forces it becomes personality and character or, in the case of animals, individuality. Although temperament is assumed to be largely a matter of inheritance, other factors such as maturation, diet, intra-uterine environment, endocrine balance, and temperature should not be overlooked as determinants of what is commonly called one's 'constitutional make-up.'

In our ignorance concerning the nature of temperament, an acceptable definition of it has yet to be formulated. As a first approximation and a working one for this review, *temperament will be defined as consisting of the emotional nature, the basic-needs structure, and the activity level of an organism.*[1] Such traits as timidity, aggressiveness, sexuality, spontaneity, variability, speed of reaction, and activity are examples of temperamental traits.

In recent years, psychologists have concerned themselves more and more with the study of personality, and much excellent work has been and is being done in this important area. Yet there is almost no information concerning the raw material out of which personality develops. The paucity of investigations of human temperament is painfully revealed by a search through the *Psychological Abstracts*. As a result, a tendency to assume that personality is a culturally determined product seems to be gaining headway among the more sociologically-minded. This tendency to ignore the basic stuff of personality arises, in part, out of an unconscious

[1] This definition and much of the material presented in the present review have been discussed in the Graduate Seminar at Western Reserve University. I am indebted to the members of this seminar and especially to Dr. C. W. Huntley for valuable suggestions and criticisms.

wish on the part of some social scientists to close their eyes to the importance of heredity in determining individual differences. These ostrich tactics are another indication that the controversy regarding nature *vs.* nurture is being transformed into a conflict between rival social, political, and economic ideologies.

Undoubtedly, the main reason for our ignorance is the difficulties involved in studying human temperament by experimental methods. At this point the animal psychologist steps in with the pertinent suggestion that investigations on animals can unearth a wealth of material concerning the nature of temperament. Individuality in animals is largely a matter of constitution. Culture plays only a minor role. Moreover, differences in the physical and social environment can be reduced to a minimum, and any number of variations in the make-up of the animal can be experimentally produced.

In the hope of stimulating still more widespread interest in this fertile field, the present writer was induced to survey the literature on temperament in animals, paying particular attention to the tools and procedures employed for measuring differences in temperamental traits. The present review is confined to those experiments making use of rats and mice as subjects. This is not a serious limitation, since these constitute the overwhelming bulk of temperamental studies. Moreover, studies of individual differences in the structure of the basic needs, *e.g.* hunger, sex, etc., have not been included. For this topic the reader is referred to those admirable studies of individual differences in the strength of drives by E. E. Anderson (1, 2).

I. Fearfulness, Timidity, Emotionality, and Wildness

These four terms, as well as such others as nervousness, agitation, excitability, instability, are grouped together because they have been used by investigators to describe similar behavior in rats and mice. They have not been operationally differentiated in any experimental studies to date. Yerkes, in an early experiment (46), did attempt to differentiate between timidity and wildness, but he concludes that "it is indeed extremely doubtful whether it (timidity) can with sufficient certainty be distinguished from wildness to render measurements significant." The present writer adopted the term 'emotionality' to describe the trait under consideration. It is, however, too inclusive, since by definition emo-

tionality denotes individual differences in all types of emotional behavior, *e.g.* anger, joy, and sadness. Timidity is perhaps the best single word for this temperamental trait, although all four, as well as others, will be used interchangeably in the present review.

A. Situations Which Have Been Employed to Evoke Timidity

1. *Handling.* The animal is removed from its home cage, held for a brief period, and returned again to its cage.

The animal is held by the root of the tail with one hand while the animal's back and head are stroked with the other hand.

The animal is released onto a screen and is immediately recaptured (**9, 39, 45, 46**).

2. *Weighing.* The animal is placed on a spring balance, its weight is noted, and then it is removed (**45**).

3. *Ear-Clipping.* Observations are made of the animal's behavior while its ears are being clipped (**9**).

4. *The Stovepipe Test.* A special apparatus was devised by Stone (**45**) to measure wildness or timidity. It consists of a U-shaped stovepipe tunnel. The sides of the U are two feet in length, and the connecting pipe is two and one-half feet. A hungry animal is placed in a starting box and runs through the stovepipe to the end-box, which contains food.

5. *The Field.* The animal is placed in an enclosure. Stone (**45**) and Yoshioka (**36**) used a square field. Hall (**18**) employed a circular field, seven feet in diameter, because it offers no corners into which the animal can retreat. As standardized by Hall and used in a number of studies (**2, 3, 4, 5, 6, 7, 9, 28, 30**), the field is well lighted by a light hanging directly over the center of the enclosure.

6. *Cage-Emergence.* The animal is given an opportunity to come out of its living cage (**3, 7**).

7. *Approach-Withdrawal.* This situation is a variation of the cage-emergence test. The experimenter walks up to the cage and notes whether the animal approaches, withdraws, or remains indifferent (**32**). Billingslea (**11**) modified this by pulling the cage forward in the rack before noting the rat's behavior.

8. *The Closed-Box, Wading Situation.* The animal is placed in a small covered metal box, $12 \times 10 \times 13$ inches. The floor of the box is flooded with one and one-fourth inches of water, in which the animal stands or wades (**3, 4, 6, 7, 8**).

9. *Swimming Tank.* This is a variation of the wading situation. The floor is flooded to a depth which makes it necessary for the animal to swim (**30**).

10. *Auditory Stimulation.* The animal is placed in a soundproof box. A loud buzzer is sounded inside of the box (**30**).

11. *The Dropped-Bucket Situation.* The animal is placed in a bucket suspended from the ceiling. The bucket is then dropped. It is not allowed to hit the floor (**30**).

12. *The Tilting-Platform Situation.* A rat is placed on a seesaw with a

glass floor. When the seesaw is tilted, the rat is thrown off balance and slides (30).

13. *The Squeeze-Box Situation.* The animal is placed in a small box with a movable side. This side is moved in until the movement of the animal is almost completely prohibited (30).

14. *The Cat Situation.* The animal is placed in the vicinity of a cat or a strong cat odor (14, 16, 24).

15. The *Straightaway.* The animal is introduced into one end of a long straightaway, and its progress down the straightaway is noted (27, 33, 40).

16. *Maze.* Sadovnikova-Koltzova (44) assumed that time to run a maze is a measure of individual differences in fearfulness.

17. *Electric Shock.* Various investigators have employed electric shock as an emotion-provoking situation. The emotion elicited is assumed to be that of fear (15, 25).

18. *Eating.* The occurrence and persistence of eating in a strange situation is observed (18, 31).

19. *The Gunshot-Eating Situation.* While the animal is eating, a gun is fired nearby (1).

20. *Loss of Weight.* The percentage drop in weight during the first three days after the animal is taken from the colony and undergoes testing is noted (33).

B. Behavior Which Has Been Assumed to Denote Timidity

1. Hiding in cage or hand (32, 39, 46).
2. Random excited activity in cage or hand (39, 45, 46).
3. Squeaking or squealing (39, 45, 46).
4. Urination and defecation (2, 3, 4, 5, 6, 7, 8, 9, 15, 17, 18, 19, 20, 21, 22, 28, 30, 36, 39, 45, 46).
5. Resistance to catching (39, 45, 46).
6. Teeth-chattering (46).
7. Cowering (46).
8. Trembling (46).
9. Distractibility, *e.g.* failure to eat in strange surroundings (1, 18, 31).
10. Jumping and hiding when released (39).
11. Muscle tension when held (45).
12. Speed of running straightaway, maze, or stovepipe (3, 7, 11, 27, 33, 40, 44, 45).
13. Agitated behavior, *e.g.* vigorous, irregular motion in running straightaway (27).
14. Freezing or inhibition of movement (14, 16, 24).
15. Reluctance to emerge from living cage (3, 7).
16. Face-washing and scratching (28).
17. Weight-loss (33).
18. Withdrawal (11, 32).

C. Quantitative Measures of Timidity

1. *Rating Scales.* Yerkes (46), Coburn (39), and Stone (45) have em-

ployed rating scales to quantify observations of wildness. Yerkes set up a scale from 0 to 5 and based his final judgment of wildness and timidity on the behavior of the rat in a variety of situations (see Section A, above). Coburn adopted Yerkes' six-point scale. Stone, using many of the same situations as Coburn and Yerkes, reduced the scale to four points, from 0 to 3.

2. *Frequency of Occurrence.* Many investigators have counted the number of occurrences of a given form of behavior during a test situation. For example, Hall (18) noted the presence or absence of defecation and urination in the field situation on each of 12 successive daily trials, two minutes in length.

3. *Time Scores.* Many of the experimental situations lend themselves to quantifying in time units. The stovepipe test, the cage-emergence situation, the straightaway, the maze, and eating may be scored in this manner.

D. Reliability of Measures of Timidity

Reliability coefficients are available for some of the measures. (In the following discussion, the test situations are numbered as in Section A, above.)

4. *The Stovepipe Test.* (Time.) Anderson (3) reports a coefficient of .95 for 91 adult male rats tested on eight separate occasions. This coefficient was computed by correlating time scores on odd days with time scores on even days and applying the Brown-Spearman formula to obtain the reliability for the total period. Billingslea (11) obtained a reliability coefficient of .94 for the last four of 10 trials in the stovepipe.

5. *The Field.* (Defecation and urination.) A variety of coefficients are available for those two measures in this test situation (3, 10, 18, 30). The lowest coefficient reported is .85. The majority of them are above .9.

6. *Cage-Emergence.* (Time.) Anderson (3) reports a reliability coefficient of .95 based on the odd-even time scores of 91 adult male rats tested on eight separate occasions.

7. *Approach-Withdrawal.* (Frequency.) Billingslea (11) obtained an R_{11} of .90 for 10 trials.

8. *The Closed-Box, Wading Situation.* (Defecation.) Anderson (3) reports a reliability coefficient of .86 for the number of fecal boluses excreted during 16 three-minute periods by 91 adult male rats.

9. *Swimming Tank.* (Defecation.) Parker (30) reports an R_{11} of .91 for male rats and .95 for female rats for 18 daily trials, 1.5 minutes per trial.

10. *Auditory Stimulation.* (Defecation.) Parker (30) reports an R_{11} of .94 for males and .91 for females for 12 trials, 3.5 minutes per trial.

11. *Dropped-Bucket Situation.* (Defecation.) Parker (30) obtained an R_{11} of .90 for males and .93 for females for 14 trials, 3.5 minutes per trial.

12. *Tilting-Platform Situation.* (Defecation.) Parker (30) found an R_{11} of .88 for males and .91 for females for 14 trials, 3.5 minutes per trial.

13. *The Squeeze-Box Situation.* (Defecation.) Parker (30) reports an R_{11} of .86 for females for 12 trials, 3.5 minutes per trial.

15. *Straightaway.* (Time.) Dawson (40) obtained reliability coeffi-
cients above .9 for the time scores of various groups of mice.

18. *The Eating Situation.* (Time.) Hall (18) reports three reliability
coefficients for the number of days during which the rat would not eat in
a strange situation. For 26 adult male rats and 26 adult female rats
tested two minutes a day for 14 days, the coefficients are .98 and .97, re-
spectively. For 31 adult male rats tested three minutes a day for 10 days,
the reliability coefficient is .88.

Thorndike (31) observed the amount of time rats would eat in a
strange situation. The reliability coefficient based on 64 adult male rats
is .79.

E. The Validity of Measures of Timidity

Many of the measures employed for studying individual dif-
ferences in fearfulness have not been carefully validated. For
example, Vaughn (33) assumed that loss of weight when rats were
taken from the colony for experimental purposes measured individ-
ual differences in wildness because some of the rats (purportedly
the wilder ones) refused to eat and excreted excessively. A number
of investigators (39, 40, 44, 45, 46) have assumed that hiding, ex-
cited agitated movement, squeaking, excretion, teeth-chattering,
cowering, trembling, muscular tension, rapid running are symp-
toms of wildness because wild rats or mice display more such be-
havior than do tame rats or mice. Although wild and tame strains
may differ in these characteristics, there is no evidence that indi-
vidual differences in timidity within a single strain are validly
measured by ratings based on the foregoing behavior items.

One acceptable method of validating any test is to obtain the
correlations between it and other purported measures of the same
trait. Hall (18) used this method in validating defecation and uri-
nation in the field situation. It has long been recognized that ex-
cretory reactions may be emotional in character, since they occur
in emotion-provoking situations, are controlled by the autonomic
nervous system, and become less frequent with repeated exposure
in a strange, fear-arousing situation. However, prior to Hall's
validation study it was not known whether variation in excretion
was a suitable yardstick for locating animals on a scale of fearful-
ness. It was necessary to prove that an animal which defecated or
urinated during many successive trials in the field was actually
more afraid than a rat which never excreted in this situation. By
correlating trials defecating and trials urinating with another as-
sumed measure of emotionality, such proof was obtained. The
other test employed was eating in the field. It was assumed, first,
that a hungry rat which would not eat in the field situation was

emotionally disturbed and, second, that the more trials in which refusal to eat occurred, the more emotionally disturbed was the animal.

The following correlation coefficients were obtained: trials defecating *vs.* trials not eating, $.82 \pm .04$;[2] trials urinating *vs.* trials not eating, $.70 \pm .06$.

These correlations are based on 31 adult male rats, tested for 10 consecutive days, three minutes a day, in the field situation. On the basis of these relationships it was concluded that defecation and urination are valid measures of individual differences in timidity or fearfulness.

Since this validation study, other relationships between various tests of timidity have been reported. Anderson (3) tested 91 adult male rats in four situations and obtained the following correlations:

Defecation in field *vs.* defecation in closed-box wading situation	$.53 \pm .05$
Defecation in field *vs.* time to emerge from stovepipe	$.43 \pm .06$
Defecation in field *vs.* time to emerge from living cage	$.49 \pm .05$
Defecation in closed-box wading situation *vs.* time to emerge from stovepipe	$.30 \pm .06$
Defecation in closed-box wading situation *vs.* time to emerge from living cage	$.26 \pm .06$
Time to emerge from stovepipe *vs.* time to emerge from living cage	$.40 \pm .06$

The defecation scores represented the number of fecal boluses excreted for 4 three-minute periods in the field and 16 three-minute periods in the closed-box wading situation.[3] None of these correlations is very high, although it will be observed that the field situation yields the closest relationships with the other variables.

Biel and O'Kelly (9) report a tetrachoric coefficient of .68 between defecation at the age of 25 days when the rat's ears were being clipped and defecation in the field at the age of 105 days. Twenty-nine rats were observed.

Vaughn's intercorrelations (33) between time in stovepipe, weight loss, and speed in a straightaway are not significantly different from zero.[4] Nor

[2] All of the measures of reliability of correlation coefficients cited in this article are P.E.'s.

[3] Anderson, in his several studies, uses fecal boluses excreted as a measure of timidity. Hall, on the other hand, scores on the basis of number of trials during which the rat defecates or urinates, irrespective of the amounts excreted. Although the correlation between the two scores is very high, the writer prefers the second one because it is not influenced, to the same extent, by such factors as amount of food eaten and time of last normal defecation.

[4] In Vaughn's experiment, practically all of the intercorrelations, except those between very closely related functions, are not reliably different from zero. In view of the fact that Vaughn's coefficients are so often out of line with those of other investigators, it is reasonable to assume that some aspect of his experimental

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did Miller and Stevenson (27) find any correlation between speed of running a straightaway and ratings of agitated behavior.

Billingslea (11) tested rats, selectively bred by Hall (see Section I, G, for a discussion of these strains) for timidity and for fearlessness, in the stovepipe and the approach-avoidance situation. The timid rats took about four times longer to run through the stovepipe (after the first two trials) than the bolder animals. In the approach-avoidance situation, the emotional animals withdrew on one-third of the occasions, while the non-emotionals seldom ran to the back of the cage.

The most complete intercorrelational study of fear-provoking situations is that carried out by M. M. Parker (30). He recorded defecation in six situations: field, auditory-stimulation, dropped-bucket, tilting-platform, swimming-tank, and squeeze-box. Table I reproduces the intercorrelations based on the defecation scores of 100 adult male and 100 adult female rats.

TABLE I

<small>INTERCORRELATIONS OF EMOTIONAL DEFECATION OF RATS IN SIX SITUATIONS</small>

(Data obtained from Parker)

		Field	Buzzer	Drop	Slide	Swim	Restraint	
					MALES			
Field			.65	.73	.63	.58	.75	
Buzzer	F	.89		.61	.69	.70	.68	
	E							M
Drop	M	.65	.65		.59	.61	.58	A
	A							L
Slide	L	.60	.85	.91		.71	.58	E
	E							S
Swim	S	.75	.76	.76	.87		.66	
Restraint		.77	.81	.79	.68	.78		

FEMALES

P.E.'s range from .01 to .04

These intercorrelations reveal a surprising consistency of individual differences in emotional defecation as evoked by quite different situations. It is evident that the trait of emotionality, at least as measured by defecation scores, is a general one.

F. Relationships Between Timidity and Other Aspects of Individuality

Individual differences in fear behavior have been studied in relation to a variety of traits. The relationship between fearful-

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procedure operated to camouflage functional relationships. Perhaps, there is a fault inherent in all intercorrelational studies the data of which are obtained by putting a group of animals through a series of tests, one after another, without regard to the effect which one test has upon the next.

ness and each of these other characteristics is discussed under a separate heading below.

1. *Activity in the Field.* It might be expected that fearful rats would be less active in the field than fearless animals, since inhibition of movement is one expression of apprehensiveness.

Hall (20) has reported small, negative correlations ($-.29 \pm .09$, $-.32 \pm .09$) between the number of days defecating and ambulatory activity in the field. (The records were obtained coincidentally.) This correlation increased to $-.67 \pm .05$ after a number of trials in the field under various conditions.

Similar correlations for three groups of rats have been found by Biel and O'Kelly (9). Group I consisted of rats with cortical lesions; Group II were operated upon, but no lesions were made; and Group III were normal rats. The correlations between days defecating and activity in the field (measured coincidentally) were $-.68 \pm .16$ for Group I, $-.32 \pm .19$ for Group II, and $-.67 \pm .14$ for Group III.

Anderson has also reported two correlations, $-.33 \pm .09$ (2) and $-.02 \pm .07$ (3) between the number of fecal boluses excreted and field activity.

A. W. Yerkes (35) noted that inbred, timid rats were more inhibited in their movements in a maze and a discrimination box than were bold, stock rats.

2. *Spontaneous Activity.* The relationship between timidity and 'spontaneous' activity is confused by three contradictory findings. Anderson (2) reports a negative correlation, $-.41 \pm .08$, between emotional defecation and the number of revolutions turned in an activity wheel during the last four weeks of an eight-week period.[5] Vaughn (33) found no relationship between the stovepipe test and voluntary activity. Billingslea (10) obtained a positive correlation, $.34 \pm .11$, between days defecating in the field and wheel activity during the last four days of a 14-day period. In two further studies, Billingslea (11, 12) found that emotional rats were considerably more active than nonemotional rats. These results have been further corroborated by Hall in unpublished findings. Since the number of animals studied by Billingslea and Hall is large, it seems safe to conclude that fearful rats are actually more active than fearless rats after a period of adjustment in the wheel.

3. *Exploratory Behavior.* Anderson (2) found no relationship between emotional defecation and each of six different tests of the strength of the exploratory drive. (The defecation score was obtained coincidentally with some of the exploratory measures.) Billingslea (10) obtained one negative and one positive correlation between days defecating in the field and two tests of exploratory behavior. Trials defecating in the field correlated $-.34 \pm .11$ with investigatory activity during the first half hour when the rats were permitted access to an activity wheel. (The living cage was connected to the wheel by a tunnel.) On the other hand, a positive correlation of $.43 \pm .10$ was found between emotional defecation in the field and exploration of a new living cage connected by a tunnel to the rat's familiar

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[5] For a discussion of Anderson's measure of activity see Section IV, C, 3, below.

living cage. Billingslea explains these contradictory results by the hypothesis that the curiosity of timid rats is actually stronger than that of fearless rats, but in a novel situation (the activity wheel) the timid rat's curiosity is inhibited by fear. On the other hand, a more or less familiar situation (the new living cage) permits their natural curiosity to express itself unhampered by apprehension.

4. *Learning.* That emotional rats make more errors and take more time in running a maze than less emotional rats has been well proved by Anderson (2). The correlation coefficients appear in the following table:

Number of Fecal Boluses Excreted *vs.*:

Maze A errors	$-.40 \pm .08$
Maze A time	$-.42 \pm .08$
Light discrimination errors	$-.28 \pm .09$
Light discrimination time	$-.40 \pm .08$
Maze B errors	$-.08 \pm .09$
Maze B time	$-.26 \pm .09$

The lower correlations for Maze B may be due to the order in which the tests were made. Maze B was the last one run by the rats, and it is possible that individual differences in timidity had virtually disappeared by that time. In line with Anderson's findings, Thorndike (31) reports a positive correlation between the amount of time spent eating in a strange situation and both maze learning and problem-box solving. (Rats eating less are assumed to be more apprehensive and therefore learn less readily.)

A. W. Yerkes, in an early experiment (35), found that timid inbred rats made more errors and took more time to learn a maze than stock animals. The following quotation from her article raises the interesting question as to what would happen to the learning ability of the timid rat if its timidity were eliminated:

They (the inbred rats) showed apparently, therefore, not less ability to form the maze habit than the stock rats but simply a greater timidity in unfamiliar situations, which hindered their forming the habit as quickly. This is borne out by the results of the discrimination experiment where the inbred rats were capable of carrying discrimination further than the stock rats, although they required more time and made more errors in doing so (pp. 289–290).

In other words, Mrs. Yerkes suggests that timid rats (if their timidity were removed) might actually be better learners than bold rats. This hypothesis is in line with Billingslea's theory mentioned in the preceding section.

Two investigations fail to substantiate these findings. Zero correlations between the stovepipe test and maze learning, problem-box solving, light-discrimination learning, and reasoning have been reported by Vaughn (33). O'Kelly (28) found no significant difference in the ability of defecating and nondefecating rats to learn a water maze. The difference, although not reliable, did reveal somewhat poorer learning on the part of defecating rats (8.6 trials *vs.* 10 trials to learn).

Although Stone (45) did not succeed in training trapped wild rats to

run a maze, he observes that half-breeds and quarter-breeds learned even faster than tame albinos.

5. *Strength of the Thirst Drive.* Anderson (2) obtained no significant relationships between fecal boluses excreted and each of 10 different tests of the thirst drive. The only correlation that approached significance is one of $-.31 \pm .09$ between speed of running a 27-foot straightaway to water reward and emotional defecation.

6. *Strength of the Hunger Drive.* Anderson (2) likewise found no correlation to exist between emotional excretion and any of 14 different tests of the hunger drive. Again, the highest one, $-.28 \pm .09$, is between defecation and speed of running to food.

Hall (19), on the other hand, showed that hungry rats were less emotional than nonhungry rats in the field situation.

7. *Strength of the Sex Drive.* Anderson, in a series of investigations, has proved beyond question that individual differences in timidity are negatively correlated with sexuality. In the first experiment (2) Anderson reports the following correlations between emotional excretion and three direct sex tests:

Number of Fecal Boluses *vs.*:
Copulation frequency	$-.44 \pm .08$
Copulation frequency (retest)	$-.40 \pm .08$
Vaginal plugs delivered	$-.50 \pm .07$

In a second study (3) Anderson employed four tests of timidity and correlated each of them with copulatory frequency and vaginal plugs delivered. These coefficients appear in the following table:

Boluses Excreted in Field *vs.*:
Copulation frequency	$-.34 \pm .06$
Vaginal plugs	$-.36 \pm .06$

Boluses Excreted in Closed-Box Wading Test *vs.*:
Copulation frequency	$-.34 \pm .06$
Vaginal plugs	$-.36 \pm .06$

Emergence From Living Cage *vs.*:
Copulation frequency	$-.51 \pm .05$
Vaginal plugs	$-.58 \pm .05$

Stovepipe Test *vs.*:
Copulations	$-.42 \pm .06$
Vaginal plugs	$-.43 \pm .06$

In a third study (5) with female rats Anderson found that animals were less timid (measured by the same four tests employed in the foregoing investigation) when sexually receptive than when not in heat. As a final corroboration of the inverse relationship between fearfulness and sexuality, Anderson and Anderson (8) report a reduction in timidity when the female sex hormone, estrogen, was injected into either normal or castrated female rats. (Estrogen presumably increases the sex drive.) Only one study is out of line with the foregoing evidence. Castrated rats should be

more timid than normal rats. Anderson (**6**) did not find this to be true. Animals castrated at about 40 days of age and tested in the field, the closed-box wading situation, and the emergence from the living cage situation at approximately 112 days of age were no more fearful than normal rats. Since Stone has found that the sex drive wanes slowly in castrated rats, it may be that the sex drive was still present in the gonad-ectomized animals.

8. *Aggressiveness.* As might be expected, timid rats tend to be non-aggressive. Billingslea (**11**) found that fearless rats engaged in many more fights than timid rats. Out of 138 opportunities to fight over food which only one rat could eat at a time, there were 4 fights engaged in by timid rats, whereas the fearless rats participated in 64 encounters.

Billingslea also found that aggressive rats ran rapidly through the stovepipe as compared with docile rats.

9. *Weight of the Endocrine Glands.* Anderson and Anderson (**7**) meas-ured individual differences in timidity by four tests, namely: time to emerge from the living cage, stovepipe test, field defecation, and closed-box defecation. They then sacrificed the rats and weighed the pituitary, thyroid, and adrenal glands and the seminal vesicles and testes. With body weight partialed out, the correlation coefficients between fearfulness and gland weights were not significantly different from zero. The 91 male rats were 309 days old at the time of post mortem.

Yeakel and Rhoades (**34**), on the other hand, did find significant dif-ferences in gland weights for two strains of rats—one timid, the other rela-tively fearless. The pituitary, adrenals, and thyroid were weighed, and in each case the glands of the emotional strain were heavier than those of the nonemotional strain. Yeakel and Rhoades worked with animals well differentiated in emotionality, whereas Anderson and Anderson employed a sample of normal laboratory rats. Furthermore, the ages of the animals at death in the two investigations differed. These two factors, as well as other dissimilarities in experimental procedure, probably account for the contradictory findings.

10. *Persistence.* Vaughn (**33**) found no correlation between the stove-pipe test and a measure of persistence. The test of persistence required the rat to attempt to tear its way through a paper window to food. Bil-lingslea (**10**) used Vaughn's persistence test and obtained a slight, but sig-nificant, positive correlation between it and trials defecating in the field. Further observations by Hall and Klein (**23**) reveal no relationship be-tween these two traits. Timid rats appear to be no more and no less persevering than fearless rats.

11. *Variability.* Emotional rats are more variable (or less stereotyped) than nonemotional rats in their responses to a five-pathway situation. All of the pathways led to food and all were equidistant (**21**).

12. *Susceptibility to Epileptic Attacks.* Martin and Hall (**26**) subjected fearful and fearless rats to air-blast stimulation in the field situation. More of the fearless rats displayed complete tonic-clonic convulsions than did fearful animals. In explaining these unexpected results, the experi-menters assume that the expression of fear by the fearful rats safeguarded them against more pathological reactions to the auditory stimulus. Ap-

parently, the rats which react with little fear to a mildly emotional stim-
ulus (the field) are more likely to be forced into epileptic attacks to a
traumatic stimulus because they have no other way of discharging emo-
tional tension. Martin and Hall point out that these results imply a nega-
tive correlation between individual differences in emotional reactions at
different levels of emotional stimulation.

13. *Inbreeding.* A. W. Yerkes (35), in comparing stock rats with
highly inbred Wistar rats, found the latter strikingly more timid than the
·former.

G. The Inheritance of Individual Differ-
ences in Fearfulness

The present writer has been engaged for several years in selec-
tively breeding two strains of rats on the basis of emotional defeca-
tion and urination scores obtained in the field situation (22). Rats
displaying no excretion are mated together, and rats defecating or
urinating for a number of trials are interbred. Eight filial genera-
tions have been obtained to date. The differences between the two
strains have increased markedly from the first to the eighth gen-
eration. The results are presented in Table II.

TABLE II

A STATISTICAL SUMMARY OF SEVEN GENERATIONS OF TWO RAT STRAINS SELEC-
TIVELY BRED FOR EMOTIONAL (E) AND UNEMOTIONAL (NE) BEHAVIOR

| | Means | | Sigmas | | |
Generation	E	NE	E	NE	Critical Ratios
P	3.86		3.54		
F_1	3.07	.46	3.36	.77	4.74
F_2	4.72	1.94	4.12	2.28	2.50
F_3	3.92	1.02	3.64	1.30	6.00
F_4	4.69	1.40	3.89	1.43	7.00
F_5	4.96	.41	3.85	1.18	8.44
F_6	‑6.87	.51	3.28	1.13	12.72
F_7	7.82	.17	3.18	.47	20.40
F_8	8.37	1.07	2.94	2.46	14.29

Means represent average number of days during which emotional defecation
and urination occurred out of a total of 12 trials in the field.

As yet no hybridizing experiments have been made so that we
do not know anything about the underlying genetic factors. It is
evident, however, that individual differences in fearfulness rest in
part on a genetic basis.

A number of breeding experiments involving both wildness and
savageness have been reported. A discussion of these investiga-

tions will be deferred until after the section on savageness and aggressiveness.

II. Aggressiveness and Savageness

The experimental literature on the topic of individual differences in the anger emotion of rats and mice is scantier than that for timidity. In view of the recent interest in aggressive behavior, a review at this time should facilitate studies in this area of temperament.

In the breeding experiments of Yerkes (46), Coburn (39), Stone (45), and Utsurikawa (32) savageness and wildness were observed coincidentally. Many of the same situations were used by these investigators to evoke anger and fear.

A. Situations Which Have Been Employed To Evoke Aggressiveness

1. *Handling,* 2. *Weighing* (39, 45, 46). (See Section I, A, for description.)
3. *Inserting an Object Into Cage.* Utsurikawa (32) evoked viciousness by pushing a wire through the mesh side of the living cage.
4. *Tail-Pinching.* A rat's tail is pinched by means of a forceps (24).
5. *Nose-Tickling.* A rat's nose is tickled with a straw (24).

In these situations the savage reactions are directed toward the experimenter. In the following situations, aggressiveness between animals is elicited.

6. *The Competitive Situation.* Two rats which have been deprived of food or water are placed together in a cage with food or water (37). Billingslea (11) employed food dishes and water bottles which permitted only one rat at a time to eat or drink.
7. *Group Shocking.* Several rats are placed in a cage floored with an electric grid through which shocks can be administered (38).

B. Reactions Assumed To Be Aggressive

1. Biting (11, 32, 37, 38, 39, 45, 46).
2. Exposing or gnashing teeth (45, 46).
3. Jumping at hand or forceps (46).
4. Squeaking or hissing (39, 45, 46).
5. Opening mouth as if to bite or strike (45).
6. Laying back ears (45).
7. Fighting (11, 37, 38).
8. Crowding or pushing other rat (37).
9. Hair erection (37).
10. Peculiar restless movements directed toward other rat (37).
11. Puffing or snorting sounds (37).

C. Quantitative Measures of Aggressiveness

1. *Rating Scales.* Yerkes (46), Coburn (39), Stone (45), Utsurikawa (32), and Davis (37) have devised rating scales to measure individual differences in aggressiveness. Davis's rating scale, based upon Items 1, 5, 8, 9, 10, and 11, above, consists of the following steps:

0 Pays no attention to other rat or rats.
1 Intermittently moves in direction of other animal; shows signs of threatened attack.
2 Keeping constantly 'after' other rat but not actually attacking him.
3 Crowding against other rat; standing over him when latter lies flat on back; no biting.
4 Behavior under (3) plus occasional attempts at biting.
5 Bites other rat sufficiently hard to evoke squealing.
6 Bites savagely and frequently; draws blood.

2. *Frequency of Occurrence.* Billingslea (11) noted the number of fights between animals for a constant number of observation periods.

D. Reliability and Validity of Measures of Aggressiveness

With the exception of a single reliability coefficient, .96, the presence or absence of fighting observed during six trials (Billingslea, 11), no data on the consistency of individual differences in aggressiveness have been reported. Moreover, there have been no attempts to validate tests for aggressiveness. Such reactions as biting or fighting possess validity *per se* and could be used as criteria against which to validate other less obvious and less direct measures of aggressiveness.

E. Relationships Between Aggressiveness and Other Traits

Aside from the inverse relationship between individual differences in timidity and aggressiveness found by Billingslea (11) and discussed in Section I, F, 8, above, there are no studies which correlate aggressiveness with other aspects of individuality. This should prove to be a fertile field for investigation.

III. THE INHERITANCE OF WILDNESS AND SAVAGENESS[6]

In 1913 Yerkes (46) reported the results of crossing wild rats with tame rats. The wild rats, which had all been trapped, were

[6] Wildness or timidity and savageness or aggressiveness are positively correlated traits in wild rats. In tame laboratory rats they are inversely related, as Billingslea has shown (see Section I, F, 8, above). It is interesting to conjecture

extremely vicious, wild, and timid when handled. The tame laboratory rats were docile and unafraid. The first filial generation was obtained by crossing wild males with tame females. Attempts to cross tame males with wild females proved unsuccessful because the wild mother killed the young. The F_2 generation was obtained by random matings of the F_1 animals.

The ratings for the first generation half-breeds and the F_2 animals on the first test were as follows:

TABLE III

PERCENTAGE OF RATS

(F_1, N = 78; F_2, N = 115)

	Savageness		Wildness		Timidity	
	F_1	F_2	F_1	F_2	F_1	F_2
0 (least)	05	53	00	02	00	02
1	10	16	01	28	04	41
2	03	07	04	32	13	39
3	06	08	12	26	10	10
4	24	10	29	06	31	03
5 (most)	51	06	54	06	42	05
Average ratings	3.92	1.27	4.33	2.27	3.97	1.88

It will be noted, first, that the half-breeds resemble their wild fathers, who received ratings of 4 and 5 on the three traits, considerably more than they do their tame mothers, who received ratings mainly of 0, and, second, that by crossing the F_1 rats at random a marked decrease in viciousness, wildness, and shyness in the F_2 resulted.

The first result points to the dominance of the genetic factors underlying savageness, wildness, and timidity over those for tameness. Moreover, the ineffectualness of the tame mother's influence on the temperament of the offspring is quite apparent.

The contrasting temperaments of the F_1 and F_2 generation cannot be safely interpreted, since the ratings for the parents of the F_2 rats are not divulged. It is possible that the F_2 generation was produced, in the main, by the more docile and tame half-breeds. In other words, the decrease in wildness of the F_2's may be the result of the unconscious selective breeding among the F_1 animals.

Environment does, however, have an influence upon these temperamental characteristics. The rats were tested from three to

how these traits got pulled apart during the selective breeding which resulted in our tame strains. Perhaps, by selective breeding, we can get back a strain of wild rats starting with tame animals. The underlying genetics is doubtless quite involved.

five times during the course of a month, and by comparing their ratings on the first and last test a marked decrease in the wild characteristics was found. In the following table such a comparison has been made for the F_1 generation.

TABLE IV

(F_1, N = 78)

	Savageness		Wildness		Timidity	
	First	Last	First	Last	First	Last
0 (least)	05	33	00	01	00	01
1	10	12	01	08	04	19
2	03	10	04	26	13	29
3	06	18	12	27	10	28
4	24	19	29	29	31	19
5 (most)	51	08	54	09	42	03
Average ratings	3.92	2.00	4.33	3.03	3.97	2.53

Taming by handling is especially marked in the case of savageness, where the average rating is reduced by one-half. By comparing these results with those presented in Table III it will be seen that a month of handling is almost as effective in changing temperament as that resulting from interbreeding F_1's.[7]

It may be that some of the reduction in the ratings of the F_2 animals was due to the increased skill in handling the rats by the experimenter.

Yerkes observed no striking differences between males and females in temperament.

The next study is that of Coburn on mice (**39**). He employed much the same procedure as Yerkes, with the exception that his

TABLE V

PERCENTAGE OF MICE

(F_1, N = 294; F_2, N = 410; F_3, N = 164)

	Wildness			Savageness		
	F_1	F_2	F_3	F_1	F_2	F_3
0 (least)	00	01	00	00	01	00
1	01		01	00		00
2	01	02	04	01	04	04
3	24	28	38	29	23	21
4	48	44	46	34	31	24
5 (most)	26	25	11	35	41	50
Average ratings	3.95	3.89	3.60	4.01	4.09	4.07

[7] This is probably not a fair comparison. The reduction in the three trials as a result of handling may be confined to the specific handling situation and the particular person doing the handling, whereas breeding may have a generalized influence on temperament apart from any specific situation or person.

mice were not rated for timidity. Wild mice of both sexes which had been trapped and were rated 4 or 5 for savageness and wildness were bred with tame mice of both sexes which were all rated 0. These half-breeds were intermated to obtain the F_2 generation. The results for the three filial generations on the first trial are presented in Table V.

Although these results corroborate Yerkes' experiment in demonstrating the almost complete dominance of savageness and wildness factors over tameness ones, they completely disagree with the earlier investigation in showing little change in these two temperamental characteristics from F_1 to F_3.

Neither Coburn's or Yerkes' findings conform to any simple genetic principles of dominant and recessive characteristics. Certainly it would not be anticipated according to Mendelian laws that self-breeding of F_1 mice and F_2 mice should fail to bring about a reduction in wildness and savageness.[8]

Wildness and savageness in mice decrease with handling, as the following table shows:

TABLE VI

AVERAGE RATINGS

| | Wildness | | Savageness | |
	First Trial	Last Trial	First Trial	Last Trial
F_1	3.95	3.16	4.01	2.45
F_2	3.89	2.81	4.09	2.13
F_3	3.60	2.98	4.07	2.91

The decrease in savageness is more marked than for wildness, which confirms Yerkes, but Coburn's mice do not show as great a reduction in either trait as did Yerkes' rats.

TABLE VII

AVERAGE RATINGS

| | Savageness | | Wildness | |
	T♀×W♂	W♀×T♂	T♀×W♂	W♀×T♂
F_1	2.70	2.93	2.06	2.42
F_1	3.14	3.16	2.21	2.73
F_2	2.57	2.40	1.96	2.04
F_2	3.54	2.60	3.55	2.77
F_3	2.22	2.58	1.72	2.54
F_3	4.00	3.36	4.27	2.81

[8] The proof of these assertions would take far too much space. Let the curious reader assume certain dominant and recessive factors and see if he can obtain any theoretical distribution which conforms to the actual results of either Coburn or Yerkes.

The influence of the temperament of the mother on the off-
spring can be evaluated by comparing the offspring of tame fathers
X wild mothers with the offspring of wild fathers X tame mothers.

A study of this table reveals no clear-cut consistent evidence
for any imitation of the mother's temperament by the offspring.

Sadovnikova-Koltzova's genetic analysis of temperament (44)
is so incompletely reported that the reader is at a loss to know
what was done, how it was done, and what happened. She as-
sumes, among other things, that time to run the Hampton court
maze is a measure of three traits: activity, fearfulness, and seeking
activity. She does report, contrary to Coburn, that hybrids (wild
X tame), when brought up with a wild father, are wild; when raised
by a tame mother, they are not savage. This statement is not sup-
ported by any quantitative evidence.

In Stone's study (45) the following groups of rats were com-
pared: (a) offspring of wild rats, (b) half-breeds (wild X albino), (c)
quarter-breeds (half-breed X albino), (d) pure albinos, and (e)
trapped wild rats which had been confined to cages and had been
given systematic handl ng for six months.⁹ Stone made no attempt to
differentiate between wildness and savageness. He rated a rat on a
scale from 0 to 3 for each of 11 reactions for 10 trials in three situa-
tions. Stone acknowledges that 6 of the reactions probably indi-
cated wildness and 5 savageness. The average ratings obtained in
Situation 1 on Trials 1, 5, and 10 for the five groups are as follows
(a high average represents greater wildness and savageness):

TABLE VIII

AVERAGE RATINGS ON SITUATION 1 (HANDLING)

	N	Day 1	Day 5	Day 10
Albinos	17	7.5	2.1	1.1
Quarter-breeds	30	9.2	2.6	1.0
Half-breeds	70	13.5	5.0	3.0
Wild (laboratory bred)	10	20.3	11.6	5.3
Trapped wild (handled for six months)	?	6.1	5.0	4.5

One of the most interesting aspects of these results is the fact
that the half-breeds obtain a score approximately midway be-
tween the score of the full wild and the albino rats, and the quarter-
breeds, in turn, are about halfway between the half-breeds and al-
binos. This is especially true for the first trial, but it also holds to a

⁹ Stone used other groups in addition to these. They are not of particular sig-
nificance to the present discussion and have therefore been omitted.

lesser extent for the other trials as well. Moreover, an analysis of
the results of Situations 2 and 3 reveals the same trend. It is ap-
parent, therefore, that Stone's findings do not corroborate those
of Yerkes and Coburn. It will be recalled that these investigators
found that wildness and savageness were dominant over tameness,
i.e. half-breeds resembled the wild parent more than the tame one.
From Stone's results it appears that either blending or a number of
factors, some dominant for wildness, some dominant for tameness,
were operating.

A comparison of the laboratory-reared wild and the trapped-
handled wild groups demonstrates that 10 consecutive days of
testing has about the same taming influence as six months of cage
confinement and unsystematic handling. Further handling of the
trapped wild group in the test situations did not markedly decrease
the magnitude of the ratings. It would be interesting to know if the
ratings of laboratory-reared wild would have decreased further had
they been tested for longer than 10 days.

Although no quantitative evidence is presented, Stone remarks
that "we do have some evidence to show that full-wild young,
when reared by albino mothers from birth, yield rating scores
which approximate the mean of their genetic strain rather than the
mean of the foster-mother or foster-litter-mates. A few albinos
similarly placed with a full-wild mother and young were not wilder
or more savage than other albinos reared in the usual way" (p. 22).

Stone found no consistent sex differences in wildness-savage-
ness in any of his groups.

Stone also tested the various groups in the stovepipe apparatus.
The percentage of animals in each group entering the food com-
partment within one minute for Trials 1, 5, and 10 is as follows
(the greater the speed the less the timidity):

TABLE IX

PERCENTAGE OF RATS

	Trial 1	Trial 5	Trial 10
Albinos	65	95	97
Quarter-breeds	32	70	92
Half-breeds	18	35	75
Wild	20	0	12

These results are in line with those obtained from the other
tests. In the same year that Stone reported his findings, Dawson
(40) published the results of an investigation on the inheritance of
wildness in mice. Starting with a strain of wild mice which had

been reared in the laboratory for several years and a strain of tame mice, Dawson made various crosses in order to determine the nature of the genetic factors determining wildness and tameness. Dawson used but a single test, namely: the time to run a 22-foot straightaway. The score was the average of the times for three trials run at weekly intervals after the mice were 75 days of age. The mice were not motivated, and retracing in the straightaway was prevented by a movable partition which followed along behind the mouse as it ran. Dawson assumed that the faster the mouse ran the wilder the mouse was. This assumption is borne out by the behavior of the wild and tame strains.

TABLE X

TIME IN SECONDS TO RUN STRAIGHTAWAY

	N	Males Mean	Sigma	N	Females Mean	Sigma
Wild	43	6.70	1.96	47	5.28	1.70
Tame	63	24.49	7.70	54	25.35	8.99

The results from various crosses appear in Table XI.

TABLE XI

TIME IN SECONDS FOR VARIOUS CROSSES

	N	Mean Males	Sigma	N	Mean Females	Sigma
F_1 Wild×tame	76	7.58	2.55	88	6.90	2.82
F_2 (F_1 interbred)	175	12.95	7.59	190	11.85	6.77
F_1×wild	26	6.58		24	6.17	
F_1×tame	54	27.39		48	18.73	

It is quite apparent, as Dawson points out, that the factors for wildness are dominant over those for tameness. Dawson assumes that two or three genes have considerable influence in determining individual differences in wildness. In addition, he believes that there are other modifying genes.

Dawson also carried on selective breeding for four generations. The fastest wild mice were bred together, and the slowest tame were mated. There is very little change in the scores of the wild strain, but a considerable increase for the tame (see Table XII).

TABLE XII

AVERAGE TIME IN SECONDS

	Wild		Tame	
	Males	Females	Males	Females
F_1	6.70	5.28	24.49	25.35
F_2	6.90	5.59	39.56	34.19
F_3	6.42	5.25	45.50	44.00
F_4	5.00	5.86	72.00	54.37

These results can only mean that the tame strain was not homozygous for tameness.

There are slight sex differences in the various groups, with the females tending to be somewhat faster than the males.

Dawson did not find any significant difference between half-breeds raised with wild mothers and half-breeds reared with tame mothers, as the following table shows:

TABLE XIII

AVERAGE TIME IN SECONDS

	F_1 Males	F_1 Females
Wild male × tame female	7.74	6.71
Tame male × wild female	7.12	7.36

The last study to be reported is that by Rasmussen (**43**). Rasmussen champions the greater dependence of wildness (shyness) upon environmental conditioning than upon germ plasm. He points to the following 'facts' which support his thesis: (1) Rats reared in large cages are noticeably wilder than rats reared in small cages; (2) rats become tamer with handling; (3) rats allowed to run freely in laboratory room become wild; (4) if the mother is shy and wild, the offspring are likely to display the same traits.

In the present article Rasmussen cites experimental evidence only for the last statement. A wild gray female rat was mated with a wild gray male. At the same time an albino male and female were mated. When the young of both litters were three days old, two of the wild litter were exchanged for two of the tame litter. Rasmussen says that the two wild rats reared by the tame mother were "*completely tame.*" "Without the slightest difficulty and at any time they may be taken out of the cage and placed on the arm, where they may sit for a while and then start climbing upon the shoulders and back again or take excursions to other parts of E's body." Rasmussen did note that these wild rats were more active than their tame foster-siblings. On the other hand, tame rats, reared by a wild mother, remain shy as long as they are with the foster mother, but after weaning they lose their timidity.

Rasmussen observes that 16 half-breeds were intermediate in wildness between the wild father and tame mother.

Rasmussen indulges in some complex theorizing, but in view of the small number of animals involved, his failure to state exactly the experimental conditions, and the fact that his results are con-

trary to those of former investigators, this speculation appears unwarranted.

In connection with the problem of the inheritance of wildness, King's prolonged breeding experiment (**41, 42**) is of considerable interest. Over a period of 14 years, 25 generations of a wild strain have been bred in the Wistar Institute laboratories. No new rats have been introduced into this strain since the original parents were trapped. King writes:

After 14 years of life in captivity the behavior of gray rats had changed so greatly that many of the obnoxious traits exhibited by the early descendants of feral animals had disappeared.

Fear of man and nervous tension decreased greatly. Two of the original traits did persist, however. When a strange rat is placed in a cage, it is promptly attacked and usually killed. Nor would a rat in the twenty-fifth generation allow itself to be picked up with the bare hands. In general, however, the rats gradually became less vicious and wild during successive generations.

Stone (**45**), in discussing King's investigation, asks this pertinent question:

What portion of the behavior change reported for these wild rats might just as well have been obtained in the first laboratory generation if they had been handled by caretakers who already possessed the same interest, skill, and innocence of fear of being bitten that was probably present by the time the tenth generation had arrived?

One might also ask just how much selective breeding had unconsciously taken place throughout the generations. It is not inconceivable that the parents of each successive generation were the least wild animals. In fact, it was observed that many of these rats (presumably the wildest) ate their young.

IV. ACTIVITY

One characteristic in which organisms differ widely is the amount of energy released in general bodily activity. Such activity has been called 'spontaneous' because it is released by internal rather than by external stimulation. Rundquist (**51**), after running hundreds of animals in activity wheels, cites a striking example of the wide individual differences which are found in this trait. A male rat ran 60 feet during 15 days in the wheel, while a female ran the equivalent of 200 miles during the same period.

It is a reasonable assumption that bodily activity is one of the basic traits of temperament. The level of an individual's activity

doubtless conditions, to a great extent, the development and expression of many other traits. Although there have been many investigations of activity, few studies have been oriented around the problem of individual differences in activity. Most of the work has been concerned with the effects of internal conditions, organ removal, and drug injection on the average activity of groups of animals. Little attention has been paid to activity as a trait and its relation to other characteristics of the normal, intact animal.

A. Methods of Measuring General Bodily Activity

Two methods have been used to measure activity in rodents, the *activity cage* and the *activity wheel*. The activity cage is mounted on springs which rest on tambours, and the movements of the animal are recorded on a slow-moving kymograph. Hunt and Schlosberg (**50**) utilized ordinary bird cages suspended by a spring from the ceiling, as suggested by Wilbur (**56**). The activity cage, although it gives an exact picture of temporal changes in activity, which the activity wheel does not, has not proved as popular with investigators as the wheel. In addition to the greater complexity of the activity cage as an apparatus, experimenters have found it difficult to quantify the kymograph records.

The basic form of the activity wheel is merely an enclosed wheel mounted on an axle. A Veeder counter, which records full revolutions of the wheel in either direction, is attached to the axle. Two procedures have been followed. One requires the animal to live in the wheel 24 hours a day. The other consists of placing the animal in a cage which is attached to the wheel by a tunnel. This arrangement permits him to enter and leave the wheel freely. No one, as yet, has made a comparative study of these two procedures to ascertain if the same trait is being measured.

In all of the studies to be reviewed here, the activity wheel was used. Billingslea (**10, 11, 12**) is the only investigator who has utilized the second procedure of allowing free entry into the wheel.

B. Reliability and Validity of Activity

Individual differences in activity are very consistent if a preliminary period of adjustment to the wheel is permitted. The length of the adjustment period need not be longer than 10 days, and a four-day test period is sufficient to yield reliability coefficients above .9, according to Billingslea. Most investigators, however, have employed longer test periods.

Obviously, the number of revolutions which the animal turns in the wheel is a direct measure of activity and possesses validity *per se*. It is not necessary to validate it against an outside criterion.

C. Relationships Between Activity and Other Traits

Although there is a relative paucity of information concerning this topic, a few definite relationships have been fairly well established.

1. *Maze Learning.* Anderson (1, 2), Thorndike (31), and Shirley (55) have found correlations of low magnitude between activity and maze learning. These coefficients reveal that active animals are somewhat better maze performers than lethargic rats (see Table XIV).

TABLE XIV

CORRELATIONS BETWEEN ACTIVITY AND MAZE LEARNING

Activity *vs.*:	*r*	Investigator
Errors (floor maze)	.30 ± .07	Thorndike (31)
Time (floor maze)	.40 ± .07	"
Errors (elevated maze)	.39 ± .07	"
Time (elevated (maze)	.37 ± .07	"
Errors (floor maze)	.48 ± .07	Anderson (1)
Time (floor maze)	.57 ± .06	"
Errors (floor maze)	.25 ± .09	Anderson (2)
Time (floor maze)	.25 ± .09	"
Errors (floor maze)	−.03 ± .09	"
Time (floor maze)	.28 ± .09	"
Errors	.12 ± .12	Shirley (55)
Time	.40 ± .10	"
Errors	.36 ± .13	"
Time	.46 ± .12	"

On the other hand, Vaughn (33) found no significant relationships between activity and performance in a simple maze, a 12-unit multiple-T maze, and a rectangular maze. Likewise, Thorndike (31) obtained correlations not significantly different from zero between two of the four mazes he used and activity. Graves (47) reports that animals selectively bred for maze brightness and dullness do not differ significantly in general activity.

Perhaps the best evidence for the existence of a small, but significant, relationship between activity and learning is that obtained by Rundquist and Heron (53). Rats which had been selectively bred for activity and inactivity for 17 generations were

tested on a highly reliable maze. The average number of errors for the active strain was 71.60 for 15 trials; for the inactive strain, 90.10. This difference of nearly 20 errors is almost five times the standard error of the difference.

It seems safe to conclude, despite some contrary evidence, that active animals have some slight advantage in maze learning.

2. *Other Forms of Learning.* Activity is not always associated with faster learning. It may hinder habit formation. Heron (**49**) compared selectively bred active and inactive animals in a discrimination situation which required the animal to press a lever to obtain food at the sound of a buzzer signal. Active animals could not seem to learn this discrimination, whereas the inactives manifested some improvement. Heron suggests that active rats were satisfying their strong drive for activity by pressing the lever without regard to whether food appeared or not. Another plausible suggestion is that the active rats were hungrier and the greater need to obtain food interfered with their 'attending' to the signal.

In contrast to these findings, Anderson (**2**) reports a slight positive relation between activity and light-discrimination learning. Activity and errors correlate $.29 \pm .11$, and activity and time, $.33 \pm .10$. Although these are not significant coefficients, they are opposite in sign from what would be expected on the basis of Heron's findings. Vaughn (**33**) failed to find a correlation between activity and light discrimination.

Thorndike (**31**) found no evidence of a significant relationship between activity and problem-box performance, and activity and conditioning. Similarly, Vaughn's investigation did not establish a relationship between problem-box learning and activity. Nor did Vaughn find a correlation between activity and the Maier reasoning test.

3. *Strength of Various Drives.* In his first study of the intercorrelations of drives, Anderson (**1**) found that activity was positively related to the strength of the following drives as measured by the Columbia Obstruction Method: hunger, thirst, sex, and exploration.

The correlations are as follows:

Activity *vs.* Number of Crossings Under
the Following Drive Conditions

Hunger	$.37 \pm .08$
Thirst	$.43 \pm .08$
Sex	$.42 \pm .08$
Exploration	$.39 \pm .08$

Furthermore, Anderson found very high correlations between the activity of the rats when unmotivated and their activity in the wheel when hungry (.86±.03), thirsty (.88±.02), and sexually aroused (.70±.05). This means that the activity of the rat relative to the group remains constant even though the motivational state of the whole group is changed.

In a second study (2) using many of the same variables plus many more, Anderson did not corroborate his earlier findings. This later study cannot be taken too seriously, however, when one understands the conditions under which activity was measured. The rats were placed in activity wheels after 236 days of testing in a variety of situations. They were slightly over a year old, and Anderson reports that while they were in the wheels many of them became ill and some died. As a result, the original 51 rats had been reduced in number to 34. Anderson himself concludes that the activity measure in this second study is inadequate.

4. *Persistence.* There is no relationship between activity and persistence, as measured by the length of time a rat will attack a paper barrier to get to food, according to Vaughn (33) and Billingslea (10).

5. *Metabolic Rate.* There is a marked correlation (.72±.09) between activity and metabolic rate according to Hall and Lindsay (48). Metabolic rate was determined by the Benedict and MacLeod apparatus, and activity was measured over a two-week period in wheels following a preliminary adjustment period. They employed 28 adult male albino rats. Following thyroidectomy, despite the fact that activity fell off by a half and metabolic rate by about a fourth, the correlation between metabolism and activity was reduced slightly to .62±.13. This finding may mean one of two things. Either individual differences in the functioning of the thyroid gland do not account for the marked relationship between activity and metabolism, or the thyroid does condition the activity level of the animal and this level persists as a habit despite the removal of the original mechanism.

Rundquist and Bellis (52) compared the respiratory metabolism of the F_{15} Minnesota strains of active and inactive rats using the Ebeling-Corey method for determining metabolism. The average number of calories per gram per hour for the two strains by sex are as follows:

	N	Active	N	Inactive
Males	29	8.25	16	6.28
Females	20	8.70	22	6.45

The differences between the means of the actives and inactives by sex are greater than four times the sigma of the difference.

On the other hand, correlations between activity and metabolic rate within strains were not significantly different from zero.

6. *Blood Sugar.* Shirley (**54**) determined the blood sugar of three groups of male rats by Folin's method and correlated these results with activity scores. The correlations are as follows:

Group I	$r = -.21 \pm .15$
Group II	$r = -.05 \pm .13$
Group III	$r = -.19 \pm .15$

Although these correlations are not reliably different from zero, they are all of the same sign, indicating that high blood sugar and low activity have a slight tendency to go together. Such a relationship appears reasonable, since active animals should use more blood sugar than lethargic rats.

7. *Brain Weight.* Shirley (**55**) found no relationship between activity as measured during the five days before a group of 29 male rats were sacrificed and total brain weight, weight of the cerebrum, and weight of the cerebellum.

8. *Body Weight.* There is no relationship between individual differences in activity and body weight (**2, 10**).

D. *The Inheritance of Activity*

In 1933, Rundquist (**51**) reported the results of selectively breeding rats for high and low levels of activity for 12 generations. Wheels 9 inches in diameter and $7\frac{1}{2}$ inches in width were used. The rats lived in these wheels for 21 days, but only the last 15 days were used to compute the score. The wheels were located in a room kept permanently dark.

Up to, and including, the parents of the F_5 generation no conscious effort was made to establish strains of active and inactive animals. Instead of keeping to family lines established in the parental generation, the breeders for each new generation were selected from the two extremes of the distribution without regard to ancestral performance. As a result, no significant difference between the offsprings of the extremes was found. After the F_4 generation, two selectively bred strains were started. Although inbreeding was not systematically adhered to, numerous brother-sister matings were made. In addition to activity, large litter size and rapid reproduction were used as criteria in selecting breeders.

The principal findings are the following: (a) The average activ-

ity of the active males does not change appreciably from the F_1 to the F_{12} generation. (b) The activity of the active females increases throughout the 12 generations. (c) The activity of the inactive males and females decreases markedly from one generation to the next. (d) The inactive strain becomes more homogeneous, while the active strain shows no greater homogeneity from the first to the twelfth generation. (e) The females of both strains are consistently more active than the males. (f) The size of the critical ratio remains fairly constant from F_9 on.

Although the genetics of activity have not been worked out, it is apparent from the data that activity is dominant over inactivity, and that the genes responsible for individual differences in activity are few in number.

From later publications (49) based on these two strains it appears that the inactive strain finally died out due to failure to reproduce itself.

V. MISCELLANEOUS TEMPERAMENTAL TRAITS

A. Persistence

Vaughn's test of persistence (33) has been referred to in earlier sections and will be described more fully now. The apparatus consists of a two-compartment wire-mesh cage. A four-inch-square window connects the two compartments. During preliminary training a hungry rat is placed in one compartment and enters the other compartment through the window in order to obtain food. On succeeding trials the rat finds one, three, and five sheets of paper (132 sheets to the pound stock) in the window, through which he must tear his way. Small holes are made in the paper to facilitate tearing. Coaxing is resorted to if the rat stalls. After eight days of this training, the test proper is made. On the test day, a hundred sheets of paper are placed in the window. The animal is observed for an hour period, and two scores are obtained: (1) the number of sheets torn through and (2) the amount of time spent in attacking the paper barrier. (This procedure, used by Billingslea, 10, is a modification of Vaughn's.)

The test is fairly reliable—.72, according to Billingslea. As mentioned in an earlier section, it does not correlate with timidity, nor is it related to activity (10). Vaughn found no significant relationships between it and various measures of learning. It seems to the present writer to tap a very important aspect of temperament and should be more fully investigated.

Hamilton and Ellis (57) devised a simple persistence test. It consists of an endless string passing through a cage. Food is attached to the string, and the rat pulls the string in order to obtain it. A measure of persistence is obtained by noting the length of time or the amount of string which a hungry rat will pull when there is no food attached to the string. This test has not been used as a measure of individual differences in persistence.

B. Speed of Reaction

Utsurikawa (32) invented a simple device for measuring the rat's speed of reaction. One end of a small box rests on legs; the other end is suspended from a spring. Movements of the rat in the box move the spring, and these vertical displacements are recorded on a kymograph. A bell is used as a stimulus, and the speed and amount of reaction to the bell are measured. Utsurikawa was interested in the comparative performance of inbred and outbred strains of rats. In this situation the inbred animals reacted more quickly and with greater intensity than the outbred strain.

Vicari (58) investigated the inheritance of reaction time in various strains of mice. Reaction time was defined as the speed with which the mouse traversed a simple two-unit maze. Two strains of mice, one with an average reaction time of 115 seconds, the other with a reaction time of 95, were crossed. The average of the F_1 was 76 seconds. From this and other crosses yielding similar results, Vicari concludes that speed of running in this situation is very likely determined by a single pair of Mendelian factors in which short reaction time is dominant over longer reaction time. Opposed to this conclusion is the fact that the average reaction time of the F_2 generation obtained by interbreeding the F_1's at random is as low as the F_1 generation. A more plausible explanation for these results is that the shorter reaction time of the F_1 and F_2 generations over the parental group is due to hybrid vigor which is usually found when strains are crossed.

A third method of measuring reaction time was employed by Graves (47). The rat was placed in a cage suspended by a coil spring from the ceiling. Shock could be administered through the floor of this cage, and the rat's bodily movement to this shock was recorded on a revolving film. According to Graves, speed of reaction does not correlate with activity in wheels, nor with strength of hunger drive as measured by the Columbia Obstruction Method. There is a slight tendency for fast reactors to make fewer errors in learning a maze.

CONCLUDING REMARKS

After examining the literature on temperamental differences in rats and mice, the writer feels that the following evaluations, suggestions, and conclusions are justified.

Timidity

1. A number of reliable and valid tests for measuring individual differences in timidity or fearfulness have been devised. Excretion in strange and frightening situations appears to be an especially good index of timidity.

2. The degree of timidity expressed by the individual rat is fairly consistent from situation to situation.

3. The timid rat is more active (activity wheel), less proficient in learning (maze), less sexual (copulation frequency), less aggressive (fighting), more variable, and less subject to epileptic attacks (air-blast) than the fearless rat.

4. A timid strain and a fearless strain of rats have been established by selective breeding.

5. The rats of the timid strain have, in general, heavier endocrines than the rats of the fearless strain.

6. Future research might have as its aims the investigation of biochemical and physiological correlates of timidity, the value of various therapeutic methods, and age changes in susceptibility to fear-provoking situations.

Aggressiveness

This significant temperamental trait has received almost no attention on the animal level. In view of the fact that excellent measures of individual differences in aggressiveness are available, it would appear to be ripe for investigation.

The Inheritance of Wildness and Savageness

Although this problem has been investigated a number of times in the past 30 years, it still remains to be clarified. More exact and carefully controlled studies are needed in order to establish the genetic basis for wildness and savageness and the influence of the environment in taming wild rodents.

Activity

Since activity can be so readily measured, it seems strange that more studies oriented around the problem of individual differences

in activity have not been made. The Minnesota selective breeding program has established active and inactive strains of rats, and we can expect much valuable information to result from comparative studies of these strains.

Other Temperamental Traits

Although successful attempts to measure persistence and speed of reaction have been made, virtually nothing has been done with these traits experimentally. Moreover, there are a number of temperamental traits which have been completely ignored. Variability, reaction strength, docility, dominance, tidiness, coöperativeness, friendliness, grief, boisterousness, spontaneity, gregariousness are only a few of those which might be named. The animal psychologist has his work cut out for him!

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ELECTROENCEPHALOGRAPHY AND PHYSIOLOGICAL PSYCHOLOGY: EVALUATION AND STATEMENT OF PROBLEM

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I

When a new technique of measurement has been introduced into any field of endeavor, four questions pertaining to it may be asked: What problems does it enable the experimenter to approach? What contributions has it made? What contributions is it likely to make? What variations in theoretical considerations may it perhaps induce?

Those who have attempted to bring the technique of electroencephalography into the fold of experimental psychology have frequently been asked to answer the first three of these very pertinent questions; and a few of us have wondered if there are answers to the fourth. Since answers to only two of them can be factual, and since answers to the others must to some extent be persuasive, it is not to be wondered at that electroencephalography is all too frequently regarded by many psychologists as a neat physiological trick (if you can do it) with very little meaning for the so-called traditional problems of psychology. The author would like to submit that the investigation of the human electroencephalogram is more than a neat physiological trick and that it may be expected to contribute to certain problems of psychology.

Those problems within psychology which electroencephalography seems most fitted to serve pertain to the description, in physiological terms, of behavioral events and the subsequent erection of concepts from these descriptions which will make possible the prediction of behavior (as used in the nonphysiological sense). This is what usually goes under the name of physiological psychology.

There appear to be at least three systematically interlinked approaches to the description of behavior[1] in physiological terms.

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[1] The key term in need of definition in a discussion of this type is the term "behavior." If the term is not arbitrarily defined so that physiological psychology is, by decree, rendered superfluous, but is instead permitted a multiordinal usage

These approaches may be schematically presented (after the analysis of Brunswik, 3) as follows: "S" is a stimulus (or configuration of stimuli); "R" is a response (or configuration of responses usually called "behavior" in the least generalized meaning of the term); intervening between these measurable (?) phenomena are a number of neurophysiological phenomena, $\phi_1, \phi_2, \phi_3, \cdots \phi_n$ (or ϕ_{1-n}), which are "responses" to "S" and are "stimuli" producing "R." In part, perhaps, some events, ϕ_t, ϕ_u, ϕ_v, etc., are responses to "S," thus altering "R." Accordingly, physiological psychology may investigate: (1) the relation between "S" and ϕ_{1-n}; (2) the relation between ϕ_{1-n} and "R"; (3) the interrelation between the neurophysiological events, as these may be conditioned by "S" and themselves condition "R."

Since nonphysiological psychology relates "R" and "S" without regard for ϕ_{1-n}, physiological psychology would appear, at least to some of us, to offer a more complete view of the events determining "R." However, it should be clearly comprehended by the physiological psychologist that the only path to a systematic physiological psychology lies parallel with, and perhaps in the same vertical and horizontal planes as, the path of what is now systematic nonphysiological psychology.

The constructs of nonphysiological psychology frequently have physiological implications, some of which are more obvious than others. These implications involve, if the constructs are meaningful, precise physiological mechanisms. These must be measurable. If and when they are measured, the nonphysiological system gains economy of formulation and the physiological observations, now being oriented toward "behavior" (in the best atomistic sense), become systematic. If the basic[2] physiological concepts are coupled

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(as seems more fruitful), "behavior" can be used as a generic term to describe any adequately defined response characteristic of tissue. Thus one can speak of the "behavior" of an axon, the "behavior" of an interneurone, the "behavior" of a motor unit; less atomistically one can speak of the "behavior" of a nerve trunk, the "behavior" of the cortex, the "behavior" of a muscle group; still less atomistically one can speak of the "behavior" of a human subject in a learning situation. All of these "behaviors" are, or are determinants of, the more generalized "behavior" usually referred to, but undefined, by the psychologist; they are the subject matter of physiological psychology.

[2] The word "basic" must be used with reservation. It is futile to state that physiological descriptions of behavior are "irreducible," or that behavioral systems must be "reduced" to physiological terms. The naïveté of theorists who make such statements will become fully apparent when the biophysicist has reduced the physiological to a physical system. (See Culler, 5, on this point.)

with behavioral (S-R) concepts, the description of more complex behavior in physiological terms becomes more meaningful, and psychology (used here as an inclusive term, not split into physiological and nonphysiological) advances by a measurable amount.

The electroencephalographic technique, which provides direct physiological measurement of the functional activity of structures in the intact human organism, is suited to contribute to the type of work outlined in the just preceding brief discussion: it can provide a measure of physiological activity, as related to "S"; it can provide a measure of one type of physiological activity related to "R"; it can provide a measure of central activity conditioned by a substratum which can be simultaneously sampled by other available techniques.

II

The electroencephalogram (EEG) is made up of a series of phenomena. While it is not a precise measure of a finite piece of cortical tissue, it is sufficiently differentiating from area to area over the cortex to lead to a fair degree of confidence in its localizing abilities. Speaking descriptively, the EEG is composed of electrical potentials which vary continuously in two dimensions: frequency and amplitude. The frequencies are relatively slow, their easily definable limits being $\frac{1}{3}$ to 50 cycles per second (although greater limits probably will appear as measurement becomes more precise). The recordable amplitudes are small, normally ranging from 1 to 100 microvolts, while the subject is awake (and as high as 5 to 800 microvolts during normal sleep).

Three general bands of frequencies appear to stand out in most records as organized rhythms: one band falls at roughly 3 per second, the *delta* band; a second falls at roughly 10 per second, the *alpha* band[3] (this value is for normal adults; the "alpha" frequency is a function of chronological age, appearing at 3 cycles during the third month and reaching 10 cycles during the eighth to tenth year of life, **39**, **48**, **61**, **62**); and a third falls at roughly 20 cycles per second, the *beta* band. In most records, alpha is outstanding, due to its relatively high voltage and its characteristic of being influenced by environmental variation. It has, indeed, come to be regarded

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[3] "Alpha" is difficult to define because of the fact that its frequency changes with age. It is possible to define it as the frequency of the rhythm that is affected by visual stimulation. If this is done, then the inconsistency of arbitrarily defining alpha as rhythm between 8 and 12 cycles per second, which is 3 cycles per second at five months of age, is escaped.

by many as the primary phenomenon of the EEG and has sometimes been set into verbal, but not factual, equation with the all-inclusive term "electroencephalogram." This is very likely the result of a dichotomy set up by early workers to describe the EEG in terms of alpha and beta (delta was described later), even though it was demonstrated as early as 1936 (**42, 69**) that frequency was a continuous, unimodal distribution. Consequently, many of the data to be reviewed treat only alpha phenomena, which merely means that not all of the electrocortical characteristics of the situation being studied have been analyzed.

The EEG exhibits two properties which appear superficially to be, but are not, antagonistic. These are *constancy*, on the one hand, and *variability*, on the other. Their definitions differ only in terms of the time intervals upon which measurement is based.

Over fairly long intervals of time the "pattern" of the EEG and, quantitatively (but more narrowly), the per cent time an alpha rhythm is present on the record tend to be repeatable for a given individual. To establish a fairly reliable measure of this constancy, relatively long intervals of record must be sampled—at least 30, and usually 90, seconds of record. There also appears to be a tendency for different persons to show different types of record, although there is enough similarity between the records of some persons to render positive identification by this means impossible. This so-called constancy may also be considered as a type of variability—an *interindividual variability*. Both qualitative and quantitative demonstrations of its existence have been amply made by Travis and Gottlober (**66, 67**), Davis and Davis (**8**), and Rubin (**57**). More substantial demonstration has been made by Grass and Gibbs (**21**) in their quantitative analysis of a continuous range of frequencies, which indicates the frequency-energy characteristics to be not too widely variant in repeated samples on the same person.

While EEG constancy is based upon long time intervals, *variability* is based upon relatively short time intervals in which the moment-to-moment fluctuations in the record are not smoothed out by averaging. This type of variability may be spoken of as *intraindividual variability*. It is the most striking phenomenon of the EEG; if it were not for this characteristic there would be little correlative research to deal with.

Each type of variability has its important aspects in both clinical and experimental investigations in which physiological states,

or events, are to be related to stimulus and/or response variables. In the ensuing evaluation, minor stress will be laid upon the clinical investigations and major stress upon the experimental investigations. This selection of emphasis is based solely upon an attempt to fit EEG data within psychology; by a shift of stance they could be fitted to clinical neurology, if this were the aim of the discussion.

INVESTIGATIONS OF INTERINDIVIDUAL VARIABILITY

The contribution of studies of interindividual variability, *i.e.* constancy, in the EEG to the field of physiological psychology appears to fall within the second approach outlined above: the relation of physiological state to response. In this case, it should be clear, the term "response" refers to an abstract, *generalized* response characteristic (a statistical average of a way of responding, describable as "intelligence" or "personality"), rather than to a *specific* response within an experimentally controlled environment.

Thus, such studies of the EEG have attempted to relate presumably constant neurophysiological indexes with presumably constant behavioral indexes. It should be most explicitly understood, however, that there are carried into these investigations two assumptions: that the neurophysiological measures are constant, and that the psychological measures are also constant.

"Intelligence" and the EEG

The correlation of intelligence and the EEG has been attempted by Kreezer (**43, 44**), Lindsley (**48**), Rahm and Williams (**56**), and Knott, Friedman, and Bardsley (**38**). Kreezer has contributed the greatest proportion of effort to this particular problem. He has divided his studies on the basis of the types of material investigated, but all of the material has been made up of mentally deficient patients. His major findings pertain to a group of Mongolian deficients and a group of nondifferentiated familial-type deficients (which apparently means that no disorder other than feeble-mindedness could be observed in this group). In order to control the factor of chronological age, apart from mental age, since the frequency of the alpha rhythm has been shown by Lindsley (**48**), Smith (**61, 62**), and Knott and Gibbs (**39**) to be a negatively accelerating function of chronological age, Kreezer has studied adult cases only, inasmuch as the frequency of alpha becomes stabilized at about 16 years CA. Further, he has included in his samples no subjects of normal or superior intelligence.

In his population of Mongolian-type deficients (43), Kreezer obtained a correlation of $-.21$ between mental age and alpha frequency, the limits of which he defined as 7.5 and 12.5 cycles per second, and a correlation of $+.35$ between per cent time alpha and mental age. In the nonphysiological type (44), the correlation between alpha frequency and mental age was reversed in sign, being $+.32$; and the correlation between per cent time alpha and mental age was $+.16$. Since the second group contained no primary physiological disorders, this correlation might be expected to hold for the normal range of intelligence.

Lindsley did not obtain any correlation between per cent time alpha and intelligence in a large population of children, varying in age. Rahm and Williams measured per cent time alpha and mental age, obtaining a nonsignificant correlation.

An examination of the available data would indicate that there is some reason to believe that a correlation, positive and small, exists between alpha frequency and intelligence level, at least at the lower end of the distribution of intelligence (in nonphysiological deficiency). Such an examination also leads to the suggestion that higher correlations might be obtained if the conditions were manipulated differently.

Kreezer, who has reported the significant correlations ("significant" at the 5% level of confidence only), has studied *adult* deficients. Since they are adults, they have a frequency range in the alpha band which is quite limited. Since they are deficients, they would have, if the correlation were perfect, a frequency range limited to half of the normal range. In terms of Kreezer's limits, this would be from an average of 10 to a lower level of 7.5 cycles per second.

Suppose, however, that the sample studied were to include higher than average mental ages. If there were a correlation of less than unity between alpha frequency and intelligence level, a greater range of talent would be provided, and a higher r should result.

Another factor, chronological age, may be important. At lower chronological ages, alpha frequency is slower than at older ages. The range of variation at any given age tends to be greater at the younger ages and smaller at physical maturity, so that there is more overlap between lower age levels than between upper age levels. On an hypothesis of multiple determination of alpha frequency, the factor of intelligence might be more influential in determining alpha frequency at lower than at higher chronological ages. Samples selected at these younger levels, if this is the case, should exhibit greater correlation between IQ and alpha frequency.

Accordingly, one would predict higher correlations with an increase in the range of intelligence levels studied and with a decrease in age.

Knott, Friedman, and Bardsley (38) have apparently demonstrated that some additional factors of this type may affect the correlation. They found, in a sample of 48 eight-year-old children, that the correlation between IQ and alpha frequency was .50 (significant at the 1% level). However, in a sample of 42 twelve-year-old children the correlation was .12 (not significant). The range of IQ's in the first group was from 170 to 30; in the second, from 153 to 56. Arguing that metabolic variation in the older group may have been active in altering the correlation, but admitting that the higher correlation in the younger group may have been due to chance, these authors suggest that further study of the chronological age variable is necessary.

Data pertaining to other than alpha frequencies have been published by Lindsley and Cutts (51). Although these are limited to a single case, they are extremely interesting. Observations were made upon a child, age 10, during recovery from encephalitis. Before the illness, the IQ was 145; during the course of recovery, IQ's of 118, 134, 148, and 145 were obtained, indicating an abrupt decrement with onset of, and a gradual series of increments during recovery from, the disease. A series of EEG records obtained during recovery indicated an initially high percentage of frequencies in the 2- to 4-per-second bands, with a gradual decrement in these values. Associated also with the course of recovery were behavior changes: during the acute phases, behavior was "very poor," with a gradual return to "good."

"Personality" and the EEG

The problem of determining whether individual differences in the EEG are or are not related to individual differences in "personality" represents another attempt to correlate two "constants," one physiological, the other psychological. This particular psychological "constant" is one which has long been striving to gain an organic seat, so that it is not unnatural that an early attempt was made to correlate these two types of individual differences.

Lemere (45) published the first observations, and he believed a relationship existed between a "good" alpha rhythm (*i.e.* a regular, fairly continuous one) and the cyclothymic personality. Conversely, a "poor" alpha rhythm seemed to be related to a schizothymic personality. Lemere's observations, however, were exceptionally qualitative, and no data pertaining to either the quantitative determination of "good" and "poor" alpha rhythm or the quantitative (or qualitative) estimations of personality were given.

Gottlober (19) attempted to remedy this lack of quantification. He studied a population of normal subjects who were qualitatively judged to be "extremely introverted" or "extremely extroverted." These subjects

filled out, in addition, a self-rating type personality test. Gottlober concluded, on the basis of the findings, that a greater proportion of extroverts showed per cent time alpha greater than 50 and a smaller proportion of extroverts showed per cent time alpha less than 50, and that there was a relationship between personality and the EEG.

These conclusions were based on a faulty statistical approach; he should have compared the proportions of extroverts *and* introverts who exhibited a per cent time alpha greater (or less) than 50, rather than the proportions of extroverts above 50 and below 50.

Henry and Knott (26) reanalyzed the Gottlober data and found this comparison to indicate the differences as "marginally significant," *i.e.* significant at less than the 5% level, but greater than the 1% level, of confidence. They also found that the more "objective" the rating of personality became (*i.e.* Gottlober's judges' ratings and the subjects' self-ratings in agreement, and self-ratings alone), the less significant the relationship appeared to be. A new population, divided with respect to introversion and extroversion solely on the basis of self-rating data, reversed the trend of the Gottlober self-rating data. Henry and Knott suggested that the "relationship" might be chance alone and offered specific suggestions for future work.[4]

Included within this area of research are those investigations which have correlated EEG variables with personality deficits of the type subsumed under the title of "behavior problems." Jasper, Solomon, and Bradley (35) were the first to report that children so diagnosed had a tendency to exhibit EEG's remarkably similar to those of epileptic populations. This finding has since been confirmed (6, 49, 50). In this instance it would appear that some aspect of the functional structuring of the cortex might be responsible for the total behavior of the organism, in that limits of performance may be thus established. Work of this type more nearly approaches a meaningful correlation procedure than that cited above.

This "long-time variable" type of correlation is, at the present time, rather distinctly limited to problems of the preceding character. The disadvantages of such research can be summed up by saying that these correlations frequently express two unknowns in terms of each other. The data of such studies can become meaningful only when the determinants of one variable or the other have been isolated. It will perhaps be through the correlation of

[4] Other work in this area has been done by Saul, Davis, and Davis (60) and is involved in a psychoanalytic approach to the correlation of the two variables. Due to the highly subjective character of psychoanalytic labels, this work, while certainly suggestive, will not be evaluated here.

physiological and psychological data in simple behavior that impetus to the solution of the final equations will come, especially if, from the study of simple behavior, a psychological system can encompass the terms "personality" and "intelligence."

The Assumption of EEG Constancy

As has been stated, the assumption basic to these correlations is that the phenomena are constant. Relative to the EEG, for instance, it is assumed that a distribution of cortical frequency, or per cent time alpha, will be repeated on subsequent occasions, regardless of the intervening activities of the population.

When this assumption is made, it follows all too quickly that the EEG is an index of inborn, genetically determined factors in the organization of the cortex, an hypothesis that has been explicitly formulated (7).

While there is some value in this assumption in so far as an explanatory principle pertinent to constitutional hypotheses is concerned, there are also certain difficulties which arise. These are two: stating the assumption in a manner that is provocative of test, and testing the assumption. Due to the multiple phenomena exhibited in a record of the electrical activity of the cortex, one must state which areas of the frequency continuum are genetically determined (if not all of them are) and which areas are modifiable by environmental manipulation (if any of them are).

The Davis formulation of hereditary determination was essentially concerned with the frequencies making up the alpha rhythm, and it was based upon observations of constancy of alpha index, similarity in the EEG's of identical twins, and similarity within family groups.

The assumption of constancy of alpha index, and its correlative assumption of inborn (constitutional) cortical characteristics, was investigated by Knott, Henry, and Hadley (41). They suggested that individual differences in the EEG which appeared in one condition of recording should also be made manifest, perhaps in other measures, in other conditions. Since Jasper and Andrews (31) had offered the hypothesis that certain rhythms appearing in the EEG during sleep were derived from rhythms appearing in the EEG during waking, Knott, *et al.* chose these two conditions for study.

They selected 10 subjects, five with a per cent time alpha above 65, five with a per cent time alpha below 5. During sleep these two groups produced electrocortical records which were essentially identical. The

"random" waves of the sleep-EEG, which according to the Jasper-Andrews hypothesis are derived from the alpha rhythm of the waking-EEG, were not distributed differently in the two groups. Although a rank order correlation indicated some relationship between the two rhythms (inferred $r = +.56$), the conclusion justifiably followed that a low per cent time alpha during waking did not indicate the capacity for the production of alpha waves, since the "alpha-derived" rhythm, *i.e.* "random," was abundantly present in sleep in the low (waking) alpha subjects.

Henry (24) has more recently extended these observations in a study of a group of subjects selected to represent a continuum of per cent time alpha. He found that, while this measure indicated a heterogeneity of the population prior to sleep (*i.e.* discriminated one subject or group of subjects from another), no measure of per cent time sleep rhythm ("low voltage," "spindles," "spindles plus random") indicated heterogeneity[5] (*i.e.* in terms of these measures the population was homogeneous); *and that the distribution of per cent time alpha after awakening in the morning was no longer heterogeneous*. Those subjects with the lowest presleep per cent time alpha increased an average of 20% *after* sleep; and those with the highest decreased an average of 19% *after* sleep.

Henry's findings are triply significant, for they suggest that no inborn "cortical constant" exists to determine the EEG during sleep; that no inborn constant exists to determine the per cent time alpha during waking, *per se*; and that per cent time alpha may be determined by "historical" factors, such as an intervening period of sleep.

These data seem to indicate, relative to the measure used (per cent time alpha), that the attempted correlation of this particular EEG variable with long-time behavioral concepts such as personality (or intelligence) cannot look for "explanation" of the results *wholly* in terms of inborn, constitutional factors. It would appear that there is some reasonable possibility that what conditions one index (the physiological) might also condition the other (the psychological). If this conclusion is further substantiated, a real extension of knowledge concerning the phenomena under examination can be expected.

While Henry's data seem to lead to a shelving of any concept which attributes the determination of alpha index to factors of causality which are constant, they do not cover the possibility that alpha index tends to be, in a general way, distributed according to genetic variables. This problem has been quantitatively approached by Gottlober (20), whose data on familial distribution of

[5] Correlations between per cent time alpha (presleep) and sleep rhythms were not significant in this sample. The Knott, *et al.* correlation reported seems likely to have been the result of the initial distribution of subjects.

alpha index do not offer any support to such an hypothesis. A qualitative analysis, in which a series of electroencephalographically trained judges attempted to "match" a given record with a series of other records which included the records of the immediate family, failed to support the contention that there is any more similarity within families than there is between (unrelated) families. In this analysis, the entire range of frequencies was used for judgment, not the alpha frequencies alone.

The fact remains, however, that the alpha rhythm is not the EEG, and whatever is concluded regarding the former does not of necessity apply to all of the latter. Abnormal EEG frequencies, such as those seen in the epilepsies, might be indicators of a constitutional condition, and relatives of patients exhibiting such behavioral disorders might, in this instance, be expected to show peculiarities within these regions of the frequency continuum. Lennox, Gibbs, and Gibbs (46) have presented substantial evidence of the presence of EEG patterns *resembling* those of clinically diagnosed epileptics in the nonepileptic, *i.e.* presumably seizure-free, relatives of patients. It seems safe to conclude on the basis of what is known at the present time that in this band of frequencies a constitutional variable has been demonstrated. It is significant that these abnormal frequencies are exhibited by nonepileptics, for it must be that these electrocortical conditions, while apparently necessary, are not sufficient for the behavioral abnormalities called epilepsy. Questions now arise concerning the sufficient conditions: Are they "constitutional" or "environmental"?[6]

The findings of Lennox, Gibbs, and Gibbs, taken together with the findings of Henry, seem to point to the fact that we are probably dealing with a multiplicity of determining factors in our studies of the EEG. This conclusion, based as it is upon data rather than conjecture, should do much to temper claims of simple causality and should enable theorists and experimentalists who make use of the technique to phrase issues in a manner conducive to their solution.

[6] Environment means here, as elsewhere in this paper, in the environment of a group of cortical neurones. Thus, the chemical condition of the blood stream, as well as other physiological variables, is a vital factor in the control of cortical frequencies (18, 47, 52). So are the conditions of other areas of the cortex, in so far as they are in neural connection with the area being studied. The term "environment" is clearly a relative term.

The Investigation of Intraindividual Variability

The moment-to-moment variability of the EEG seems to have a greater contribution to make to the study of the relation of physiological and behavioral variables than does its long-time constancy. Instead of being forced by the constancy view into research dealing with long-time concepts involving correlations of persons, one can apply the EEG in research involving moment-to-moment behavioral shifts, or in research involving a comparison of different "categories" of behavior along (what is really) a continuum. The fact that the EEG represents two continua, frequency and amplitude, may thus be used to advantage, for we can attempt to correlate quantitatively determined points on the physiological scale with points on the behavioral scale.

Gibbs, who originally proposed the continuum, or spectrum, concept of the EEG (15), has worked out a rough correlation of this nature, involving the extremes of behavior, *i.e.* abnormal types. For instance, fast frequencies and high amplitudes are the electrocortical equivalents of grand mal epilepsy; while slow frequencies in certain bands and high amplitudes are the electrocortical equivalents of petit mal and psychomotor epilepsy. Within the normal range of behavior this type of analysis has not been systematically worked out; but data in existence, even though pertaining to the narrow alpha band, point very clearly to a probably profitable series of investigations. Beyond the alpha band it has been found that "reduced levels of consciousness" are associated with slow frequencies (9) and that "attention" is related to fast frequencies (16). The problem to be attacked is that of correlating such frequency shifts with a quantitative behavioral dimension.

In the resting state of the individual, the EEG is made up of a constantly changing series of phenomena. To restrict this description to the alpha band, since most work has been limited to it, the alpha rhythm appears to wax and wane throughout a period of recording, even though the subject is removed, as far as is possible, from all punctate external stimulation, *i.e.* the subject is in a comfortable position, in a dark, either sound-proof or sound-masked room, with eyes closed.

The EEG and Stimulus

Variability in the alpha rhythm may be experimentally induced by a wide variety of stimuli which are differentially effective

in producing such variation. The usual response to an introduced
environmental variable is, after a latent period, a fairly rapid de-
pression of the alpha rhythm, which may reach zero amplitude or
be displaced by other frequencies. This state ensues, outlasting
brief stimuli and declining with prolonged stimuli. The magnitude
of the change varies as the sense mode is varied (the potency of
stimuli being, in order, visual, auditory, tactual), and within a
given sense mode it varies as a function of intensity, duration (4),
frequency (*i.e.* "adapting" with increased numbers of stimulus
presentations, 70), and what may be termed "complexity" (71).

The intensity of the stimulus applied has been studied most
systematically in the case of vision. Within this modality it has
been found that the latent time of blocking of the alpha rhythm,
i.e. the temporal interval between the presentation of the stimulus
and the maximum depression of the rhythm, is a function of the
intensity of the light stimulus. The duration of the alpha de-
pression, in the case of brief stimuli, is likewise a function of the
intensity of the stimulus (30), more intense stimuli producing a
greater time of total depression than weaker stimuli.[7]

The duration of the stimulus has also been found to be a factor
conditioning both the latent time of blocking of the alpha rhythm
and the recovery time of alpha (30).

The duration of the blocking is a function not only of stimulus
intensity and the stimulus duration, but of the serial order of the
stimulus in a group of stimuli of constant intensity. As a stimulus
is repeatedly presented, the duration of the block becomes less.
The function is asymptotic, apparently, reaching a minimal value
for a given experimental session[8] (although the variation about the
mean is fairly large because of uncontrolled factors).

[7] A more precise method of analysis is now being attempted by H. D. Hadley
and the author. This method involves the determination of the rate of change of
alpha amplitudes during and following stimulation. The importance of this problem
to S-R theories of behavior is to be found in Hull's postulate concerning the "stim-
ulus trace." Assuming that the blocking of the alpha rhythm is an indicator of cen-
tral changes (is a "physiological litmus paper," as it were), it should be possible to
construct empirical equations from observations of this blocking and, by suitable
methodological procedures, then to correlate these with S-R systems. By so doing,
the predictive value of neurophysiological concepts may be greatly increased in
scope.

[8] In unpublished observations the author has noted a tendency for this phe-
nomenon to be continuous through a number of experimental sessions. A point
may be reached at which it is difficult to measure reliably the latent time of block-
ing to such a presumably "potent" stimulus as light.

While the influence of *rate* of stimulation has not been studied, this factor is of undoubted importance. Henry and the author have observed in an unpublished study that when there is marked adaptation of the blocking response to an auditory stimulus (180-cycles-per-second tone) there is recovery of the response after an elapsed interval of time during which no stimuli are presented. We have also found (40) that, after marked adaptation of the alpha response to tone, the introduction of a new stimulus (changing the environmental pattern) leads to recovery of the response.[9]

This introduction of new stimuli involves the factor referred to above as "complexity." This factor has been rather inferentially studied in three ways.

Martinson, working in this laboratory, found that tone, usually reported to be "ineffective" in blocking alpha due to the rapid adaptation of its response, can be made more effective by presenting it at two intensities and presenting these two intensities randomly with two intensities of light. Travis and Knott (71) found that the visual presentation of meaningful stimuli (words) produced a longer perseveration time than light alone (intensity kept constant). They attempted to control the meaningfulness of the patterned stimuli (letter-combination) by presenting scrambled letter-combinations of the words, but their subjects tried to reconstruct the words, despite instructions to "remain neutral" to the stimuli; consequently the perseveration times were longer than for light alone and slightly, but not significantly, longer than for the meaningful words.

Knott (37) investigated the latent time of blocking to light when light was a signal for manual response. It was found that in such contexts the mean latent time of blocking was shorter than when light had no response value and that the S. D. of the distribution was also smaller. The reduced mean time was at least partly the result of proprioceptive stimulation resulting from muscular adjustment in the set to respond and could be *approximately* reproduced by experimental loading of muscle groups. The S. D. was not decreased by this loading and was inferred to be due to central factors initiated by the instructions and maintained by intra-cerebral rather than by peripheral mechanisms.

Bakes (2), in correlated research, found that reductions in the mean latent time of blocking of alpha, and the S. D. of the distribution, also appeared when an auditory stimulus was the signal for response. He did not carry out the elaborate controls of artificial muscle loading, but it seems reasonable to suppose that the reduction in the mean latent time was a function of the proprioceptive activity associated with the tensions exhibited in set (12). Bakes also found significant correlations between

[9] As we have pointed out, this may provide a measure of inhibition and disinhibition. The inference is that in this case the measure is made fairly directly upon cortical tissue and without the involvement of other mechanisms lying between stimulus and response, *i.e.* mechanisms on the motor side of the arc.

reaction time and mean latent time of blocking, which were not found in the investigations of the response to light. The difference between the two studies in this respect may perhaps be due to differences in the two neurophysiological mechanisms studied.

Since environmental variation conditions energy fluctuation of the alpha band, "spontaneous" fluctuations in the absence of observable external variation may be the result of some internal variation.

Travis (63) has presented the only data pertaining to the relationship between such variations in alpha energy and variations in "mental process." He recorded the occipital EEG on an ink-writing oscillograph and verbally signaled the subject, at times selected by inspection of the record, to give an oral report of the "conscious state" which the signal to report interrupted. The signals were presented at times when the alpha waves were large, average, and small. The "mental processes" were then categorically defined, and the percentage of large, average, or small amplitude samples in each category determined. Some of these data are summarized in the following tabulation.

"Mental Process"	Small Alpha % N	Average Alpha % N	Large Alpha % N	N
Visual image ("light")	93	7	0	49
Visual image ("objects")	66	20	14	172
Auditory image	36	57	7	14
Anticipation (of signal)	45	45	10	9
Abstract thinking	21	33	46	92
"Mental effort"	67	20	13	15
"Verbal idea"	88	12	0	8
"Mental blankness"	1	9	90	118

Thus there appears to have been demonstrated a relationship between certain "types" of mental activities and certain types (large, average, small) of alpha rhythm.[10]

An experimental approach to this problem may also be made by using conditioning techniques. This approach, while not making use of introspective reports and hence not having any relationship to concepts such as "consciousness," enables one to evaluate the hypothesis that intraindividual variation in the EEG is maintained, at least in part, by intracortical mechanisms.

Loomis, Harvey, and Hobart (53) were the first to report that an auditory stimulus to which the subject had been adapted could again be made effective in blocking the alpha rhythm by pairing it with a visual stimulus.

[10] The indicated research to follow would involve a more quantitative measure of alpha energy and a quantitative measure of frequency change, in order that the physiological continuum could be scaled.

Jasper and Cruikshank (32) and Cruikshank (4) observed that an auditory stimulus which served as a signal for the subject to prepare for response to a visual stimulus acquired the characteristic of effectively blocking the alpha rhythm. Travis and Egan (64) studied this phenomenon in a more systematic manner and concluded that the blocking response of the alpha rhythm to a visual stimulus could become conditioned to an auditory stimulus.

Their results, however, were not entirely unequivocal, since it appeared that a sensitizing of the original response of the alpha rhythm to tone could have been operating. Their data, as presented, did not exhibit a curve of acquisition usually found in conditioning experimentation, the increase in alpha blocking to the auditory stimulus during pairing with light being quite abrupt.

Knott and Henry (40) attempted to differentiate this sensitization from "true" conditioning by studying delayed conditioned responses, with delay intervals of sufficient length to allow full recovery of the alpha rhythm after its response to the auditory stimulus; thus anticipatory responses to the visual stimulus should be measurable, if present. These results, too, were somewhat equivocal, for it was found that the conditioned blocking of the alpha rhythm was not only exceptionally labile, but it was extremely susceptible to inhibition of reinforcement. However, there appeared to be sufficient discrimination between an observed sensitized response to the auditory stimulus and an anticipatory response to the visual stimulus to permit the tentative conclusion that the blocking of alpha can be conditioned, in the usual sense of the term. This would mean that intraindividual variability in the EEG can occur without known peripheral stimuli being present.[11]

Jasper and Shagass (33) have apparently confirmed and extended these observations. They investigated simple, cyclic, delayed, trace, differential, differential delayed, and backward conditioning. Since their data were rather dramatically portrayed as reproductions of the original records, comment on the procedure and analysis of data seems desirable. They followed the usual routine of pairing tone (C.S.) with light(U.S.). Jasper and Shagass, however, reported that tone did not depress alpha after "5 or 6 control trials with sound alone were made" (p. 374). (This is in opposition to the findings of Travis and Egan, and Knott and Henry.) The criterion of conditioning used by Jasper and Shagass

[11] In a series of preliminary experiments we found that short trace responses could be conditioned, although they were exceptionally labile.

seems to have been remarkably lenient: "Conditioning was considered established when at least two consecutive responses to the conditioned stimulus occurred in which there was a sufficiently long depression of the alpha rhythm to be clearly a response to the stimulus rather than a 'spontaneous' variation" (p. 375). It is unfortunate that an attempt was not made in the direction of quantifying this criterion and expressing the results in continuous graphic form. This qualitative evaluation of the phenomenon, coupled with the presentation of data by the method of selected illustrations, makes it appear that there can be little question but that alpha blocking can be easily and clearly conditioned, a conclusion which Knott and Henry indicated to be contrary to the experimental events. Actually, careful perusal of the Jasper and Shagass paper seems to make it clear that there is essentially no difference in the results of the two investigations, aside from the greater number of types of response studied by Jasper and Shagass. This added information is, it should be pointed out, exceptionally vital to the full interpretation of the theoretical suggestions of Knott and Henry.

There remains, however, an important qualifying clause to be brought to a value well above the threshold of intellectual discrimination. Jasper and Shagass did *not* study the "electrical activity of the cortical centers themselves" (pp. 373–374), an assumption probably having origin in their statement that "the occipital alpha rhythm is independent of peripheral effectors" (p. 374). The data of Bakes and of Knott would lead to the conclusion that peripheral effectors have a not too far removed influence upon the occipital alpha rhythm, *via* proprioceptive channels. Since Jasper and Shagass introduced a significant variation in the procedure followed by previous workers, in requiring a manual response to U.S. ("because, in preliminary experiments, it was found extremely difficult to obtain a clearly visible C.R. when the subject remained passive," p. 374, footnote 1), it appears that their results should be evaluated in terms of both central and peripheral factors. Inasmuch as there were no quantitative measures of the peripheral events which transpired, a clear evaluation of the relative role of each factor is not possible.

In a second paper (34), Jasper and Shagass have reported that, in conditioned alpha blocking to a voluntary stimulus (*i.e.* subject-controlled: subvocal repetition of the word "block," and simultaneous pushing of a button which controlled the light

stimulus), "a zero or negative latency with respect to the manual signal was often observed." They then shrewdly comment: "This probably indicates that the voluntary stimulus, namely, the subvocal 'block,' and preparation for the manual response (proprioceptive), were sufficient to produce the C.R." (p. 506). Thus, on the basis of their own data, one may question whether or not Jasper and Shagass conditioned "the cortical centers themselves." The role of proprioceptive impulses as they are related to intracerebral processes has still to be investigated in the evaluation of all data dealing with this problem.

Another approach to this problem may be made by imposing continuous series of stimuli upon subjects and determining the relationship between the stimuli and the EEG. Two studies of this type have appeared.

Knott (36) measured the frequency of the EEG in terms of total number of potential changes per unit time ($=1$ second) in four conditions: eyes closed; eyes open, reading silently; eyes open, reading orally; and a second condition of eyes closed. The mean number of waves per second was then computed for each condition. These values are presented in the following tabulation.

Eyes closed$_1$	AM $=10.8$	SD $=2.0$	N $=1043$
Silent reading	AM $=13.3$	SD $=3.3$	N $=\ 987$
Oral reading	AM $=15.0$	SD $=5.3$	N $=\ 930$
Eyes closed$_2$	AM $=10.9$	SD $=1.3$	N $=\ 693$

The increases in means in the reading periods, compared with the mean in the initial control period, were statistically significant. The slight increase in the final, compared with the initial, control period was not significant. Graphic representation of the data, in which per cent N was plotted against waves per second, indicated the systematic shifts toward faster frequencies to be quite clear.

Relative to the correlation between the stimulus, the observed electrocortical state, and the response, complex stimulation (S) increased the frequencies of the EEG when the complex response (R) was minimal for the experimental conditions. When R became maximal for the conditions (and S presumably remained the same ?), the frequencies were still further increased.

These data may be interpreted in the following manner: If the assumption is made that the order of experimental events, which appears to represent an increase in S-R complexity, represents an order of complexity of the functional structuring of the cortex,[12] then more complex events and increased frequencies are

[12] Functional structuring is a concept introduced (37) to assist in the interpretation of the electrical activity of the cortex. "Its meaning is essentially

correlates. This tends to be supported by the fact that lowered states of consciousness (decrease in the complexity of possible response) are correlated with decrease in cortical frequency (9).

Travis and Egan (65) have reported data similar to these, but pertaining to complex auditory stimulation (speech), with no known change in *response* complexity. Measurements of frequency changes within the alpha band (only) were made. Their data are presented in terms of the percentage of the total number of seconds in which a particular frequency of alpha was observed:

	No. of Alpha Waves per Second					Mean No.
	8	9	10	11	12	per Second
% N, Silence	.9	15.7	52.3	30.2	.9	10.15
% N, Stimulation	.1	2.5	35.2	51.0	11.2	10.72

The mean increase was small, but statistically reliable.

These data, granted the fundamental assumption expressed with relation to the interpretation of the just previously cited data, again indicate a relationship between complexity of cortical events and electroencephalographic frequency.

The EEG and Response

Studies of the intraindividual variability of the EEG also have led to other approaches to problems relating the physiological correlates of stimulus and response, whereby the underlying central state is sampled and prediction of R to critical S's is attempted from knowledge of this state.

Hoagland and his co-workers (27), by measuring a characteristic of the EEG frequency continuum in the bands slower than alpha, have used their index as a predictive device to foretell variability in response. By obtaining the delta index of the EEG they discovered that the average delta index of a schizophrenic population was greater than the average delta index of a normal population. The measure is made, in a meter of record, of the length of the record in which frequencies of alpha or faster appear, plus the length of the contours of slower than alpha frequencies, minus one meter; this is a value greater than one, unless the entire meter is made up of alpha and faster than alpha frequencies. Further, within the schizophrenic group, and considering individual cases, the delta index was lower on the "good" days of a patient and higher on the "bad" days. In several instances the "bad" days could be predicted in advance by the rise in the index. In many ways this approach is similar to the investigations of IQ and personality reviewed above: it relates a central state to a response trait; it does not, however, assume "constancy" of either.

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literal, since it is difficult to think of 'function' and of 'structure' as discrete entities in the living organism. Extending the concept of structure down to the infra-microscopic, it is assumed that functional cortical events are, essentially, structural events" (p. 386).

These early clinical studies did not relate the stimulus to the central state. In later work this group of investigators studied the effects of complex stimulation ("emotional probing") eliciting complex responses upon the production of slower than alpha (delta) frequencies (28). The correlations were made upon a group of normal subjects and a group of psychopathic patients. The results clearly indicated that, in both groups, delta frequencies were a response to such stimuli and that "emotional behavior" followed the delta frequencies. Further work demonstrated that the cortical changes were apparently a function of subcortical (hypothalamic) mechanisms (29).

Gibbs and his co-workers have, by measuring frequency changes in certain specific frequency bands, been able to predict the probable time of occurrence of epileptic seizures (17) and, by varying the stimuli which are a part of the environment of the cortex (*e.g.* stimuli carried in the blood), have been able to alter both the EEG frequency characteristics and the occurrence of the seizures.

Both of these investigatory procedures represent successful attempts to use the EEG as a measure of a neurophysiological state, intervening between S and R, which can serve to predict R (even though the prediction is only roughly differential: emotional or not, grand mal or not, petit mal or not; refinement is necessary, but is to be expected).

The Relation of EEG and Other Physiological Measures

The use of the EEG as one of several simultaneously secured physiological measures makes possible the study of relations between various organic processes intervening between "S" and "R." It is this type of investigation which may pay the greatest dividends to systematic physiological psychology.

Three studies which are pertinent to this discussion have appeared. J. M. Hadley concomitantly recorded the EEG, the electromyogram (EMG), and the electrocardiogram (EKG) in an attempt to establish some relationships between central and peripheral activity. This investigation was divided into two subattacks, one during minimal psychological activity (22), the other during supraminimal, but presumably submaximal, psychological activity (23). In both, the measures used were EEG frequency and amplitude in the left motor (LM) and left occipital (LO) areas, EKG frequency, and EMG (forearm) amplitude.

During minimal stimulation (subject reclining, room dark, quiet), there appeared to be no relationship between EEG and EMG variables or between EKG and EMG variables. The correlation between LO frequency and EMG amplitude was .17; between LM and EMG, .10. EEG amplitude and EKG frequency were correlated to some extent. However, there seemed to be some tendency for those seconds of record which average 13 microvolts or greater in EMG amplitude to be associated with higher EEG frequencies, the LO:EMG correlation being .34, LM:EMG being .36.

One might be led from this to expect that interindividual variability in EEG frequency and in EMG amplitude might be correlated, but there was no relation between the magnitude or sign of the correlation in individual cases and EMG amplitude.

Although interindividual variability in the EEG dimension of frequency did not appear to be a function of muscle tension, it was still possible that intraindividual variability was. To test this, Hadley measured the EEG variables during "mental work," which is known to increase muscle tension without directly loading the muscles (10, 11).

Following the usual technique in such approaches, the subjects were presented with groups of arithmetical problems, each group of problems being more "difficult" than that preceding. These problems were solved "mentally," *i.e.* without benefit of manual devices. The EMG data substantially verified those of R. C. Davis (10, 11), which have indicated a positive relationship between "difficulty" and EMG amplitude. There was no parallel between EEG frequency, as measured, and "difficulty," although there was a sharp increase above the value during "rest." The frequency remained approximately constant throughout the work period, declining to the original rest level after completion of the problems. The correlations between EEG frequency and EMG amplitude did not differ significantly from those secured during minimal activity.

The per cent of the *final* "rest" values of the EEG and EMG measures attained during "work" are presented in the following tabulation, which will make clear the points which are significant in the evaluation of these data.

	Occiput Frequency	Motor Frequency	Muscle Amplitude
Rest (before work)	99	106	80
a × de	118	116	103
ab × de	119	115	110
ab × def	118	121	122
abc × def	119	119	152
Rest (after work)	100	100	100

This method of scoring the data makes possible two different interpretations of the results. At first glance it would appear that there is a "better" relationship between the peripheral process of muscular tension and the factor of difficulty than between the central process of number of waves per second in the EEG records and the factor of difficulty. However, it is to be observed that the ratios between final rest level tension and work level tension associated with problems of first-order (least) difficulty are *lower* than similar ratios for either of the EEG measures. Similar results appear at the second order of difficulty. This would serve as a basis

for partial refutation of a strict peripheral interpretation of psychological process.

These data seem to suggest that, on the least difficult groups of problems, the EEG is a "better" indicator of physiological change during psychological activity than is muscle tension.

If, however, the work/rest ratios are computed on the basis of *initial* rather than final rest values, the inferences from the data are somewhat modified. These percentage values are:

	Occiput Frequency	Motor Frequency	Muscle Amplitude
Rest (before work)	100	100	100
a×de	117	111	128
ab×de	118	111	140
ab×def	118	116	153
abc×def	119	114	190
Rest (after work)	101	98	111

It thus appears that throughout the periods of work there is a "better" correspondence between EMG and difficulty than between EEG and difficulty, although it is interesting that the EEG scores returned to rest values more rapidly upon cessation of work than did the EMG scores.

A third method of obtaining work/rest ratios can be computed from *average* rest values. These, expressed as percentages, are:

	Occiput Frequency	Motor Frequency	Muscle Amplitude
Rest (before work)	98	102	90
a×de	117	113	116
ab×de	118	113	124
ab×def	117	118	138
abc×def	119	116	172
Rest (after work)	101	98	112

In the least difficult work level, the increment above average rest is essentially identical in the central and peripheral measures; above this level, EMG corresponds to difficulty, while EEG does not.

These data in many ways appear to offer substantiation for the view that peripheral physiological processes are more adequate indexes of psychological processes than are central physiological processes. The defense of central processes must rest here on an audit of the mechanisms involved in the particular psychological processes being treated. If "easy" and "difficult" tasks were to be discriminated, not in terms of *kind*, but in terms of *degree*, non-

cumulative physiological indexes would show an apparent constancy, while cumulative measures might be expected to show increments. The relatively unchanged EEG measures would, therefore, be interpreted as being nondiscriminatory unless a time factor were included. It is fairly obvious that the Hadley data, while indicating that there is not a direct correlation between central and peripheral variables, have not tapped the entire repertoire of approaches to the major problem, which is the relative importance of the two types of phenomena during complex behavior. However, one can ask on the basis of these data precisely what the peripheral data do represent by way of conditions *necessary* for the progression of "mental work."[12]

A third study attempting to relate central and peripheral phenomena by means of the EEG and EMG techniques was apparently motivated by a desire to establish causal relationships between the two series of phenomena, whereas Hadley was primarily interested in discovering the degree of existing relationship and the relation between each measure and "response" to "stimulus."

Freeman (14) recorded EEG, EMG, and galvanic skin resistance (GSR) simultaneously under several environmental conditions. He found that during relaxation, which resulted in drowsiness, the alpha index of the EEG decreased, the EMG tension decreased, and the GSR increased. When this minimal condition was altered by delivering a series of increasingly intense electric shocks to the subject, the alpha index increased, the EMG tension increased, and the GSR decreased. Freeman's published data are not so extensively presented that they can be easily followed, but a "typical" graph is plotted (from data on one subject); and he believes these changes to be so consistent that he has erected an hypothesis to

[12] It is not within the scope of this paper to evaluate extensively either peripheral theory or peripheral data, but the following points might well be brought out: According to Davis, (a) muscle tension measures show a decrement with practice of the general task (of adding numbers), although the specific task (the numbers to be added) is changed, and (b) there is a significant negative correlation between successful solutions and muscle tension (10). These observations could be interpreted to mean that muscle tension, *per se*, bears no relationship to the nonmanual manipulation of symbols (mental work), but is instead related as an organic artifact, perhaps related to factors in the total performance which are not necessary to completion of the task.

While this may sound like heresy to some, the notion might possibly be worthy of experimental consideration. If practice results in a decrement of muscle tension it is conceivable that with sufficient practice the most difficult tasks would have a tension index no greater than that representing the least difficult tasks. If this is demonstrated to be the case, the EMG data would parallel the EEG data of difficulty and hence suggest that intracortical mechanisms are more efficient adapters (in work) than peripheral mechanisms.

"explain" the EEG changes as a result of changes in muscle tension and their associated changes in proprioceptive stimulation. This persuasive argument is based on assumptions that are neither tenable nor untenable; they have not been phrased with sufficient explicitness to be testable.

However, the counterproposals (a) that the muscle tension level is determined by cortical events[14] and (b) that Freeman's data are at least partially fortuitous can both be offered and supported. Hypothesis (a) could be negated only by demonstrating that myogenic control of initial tension in response to shock is predominant. Hypothesis (b) would account for the facts, as observed, in the following manner. There is no interindividual relation between alpha index and muscle tension during rest (Freeman). During relaxation culminating in drowsiness, both measures decline according to their own laws, but appear to decline relatedly. After shock is applied, the alpha index increases as the subject recovers wakefulness, and muscle tension increases as the subject becomes alert, perhaps anticipating still stronger shock; but, as no further shocks are applied, relaxation is resumed, with similar events taking place. Little importance can be laid to the fact that both alpha index and muscle tension increase, just after shock, to values greater than the predrowsiness relaxation, since alpha index values are known to deviate within 15 per cent time alpha (Rubin, 58). Henry's findings (24) would also throw the meaningfulness of these data open to question. Thus, while both rise and fall of the two measures would *appear* to be highly related, they would not be. A more precise test of this hypothesis would involve measurement of the variables in terms of short intervals of time, as was done by Hadley.

The previously cited work on the alteration of the latent time of blocking of the alpha rhythm (2, 37), in which a "set" to respond to a stimulus, visual or auditory, decreased the latent time of blocking to that stimulus, and in which artificial loading of muscle groups was observed in the case of visual stimuli to reduce the latent time, bears significantly on this general issue. In this instance there is reasonable evidence in support of the hypothesis that peripheral (sensory) factors can operate to alter a central con-

[14] In some quarters (Freeman, 13) there has been a tendency to argue that the statement of causality in this instance approaches the traditional problem of "which comes first, the chicken or the egg?" Phylogenetically, at least, the egg came first. Rather than fog the issue with analogy, it would appear possible to index each movement or each movement-produced stimulus. In this way scientific methodology can be employed to arrive at meaningful hypotheses.

dition, apparently "sensitizing" and thus assisting in more rapid cortical responses. Since it has been shown by Davis (12) that the muscular tension in set is proportional to the speed of the response in the execution of the set, Bakes' significant correlations between reaction time and latent time of blocking may possibly be attributed to a greater sensitization in the cases of greater tension, *i.e.* greater sensory stimulation *via* the proprioceptive pathways. This hypothesis should receive direct test, of course, before the inferences are made into conclusions.

The peripheral factors were not found to account for *all* of the central phenomena observed in the EEG-reaction time experiments; artificial muscle loading did *not* lead to a reduction in the S. D. of blocking times. Since this fact has been conveniently overlooked in a discussion of the role of central *vs.* peripheral factors during set (13, p. 626), it is emphasized here that intracerebral factors may also be important. Whereas the sensory factors had presumably *sensitizing* effects, the intracerebral factors had presumably *stabilizing* effects. This hypothesis, clearly preliminary and in need of further investigation, may be important in the evaluation of the "locus" of the processes called set (54, 55).

III

The just preceding review of some of the data obtained by the EEG technique (those which seemed to the author to have the greatest bearing on the problem at hand, but which, as far as is known, are not contradicted by other data) would seem to indicate that, while there is a lack of finality about them, they carry with them suggestions of new paths of research and of new interpretations of events intervening between "S" and "R."

The development of new paths of research depends upon the ease with which experimental programs may be cross- (or self-) fertilized, which in turn is partially a function of the systematic ideology of behavior to which the programs find themselves committed. Physiological hypotheses offered by rigid systematists are frequently bent by preconceived notions of what *must* happen within the organism, according to the system. As an alternative, it might be more meaningful to base physiologically phrased interpretations of behavior upon physiologically observable events.

The fact that the EEG partially opens the door which has long been locked upon the central nervous system is fairly certain to make some imprint, even though it be initially faint, upon psycho-

logical theory; for what can be deduced about the functions of the nervous system from evidence provided by this particular technique, and especially by judicious combinations of techniques, shall have to be considered when hypotheses regarding neural events are being formulated or when more formal theories are seeking referents for their postulates.

In order to provide a schema within which nonphysiological psychology may find neural correlates of behavior, it might be an acceptable procedure to steer as clear of nonphysiological entanglements as is possible and to erect a framework within which the various data of physiological psychology can be fitted. In this way psychophysiological concepts will not be judged and found guilty solely on the basis of circumstantial evidence, but largely on the basis of direct experimentation.

To this end, the following four propositions are offered. They do not constitute a theory, but are an outgrowth of the various findings which have appeared and which, when viewed together, seem to fit into this summary pattern.

(A) The EEG serves as an index of neural limits of behavior, which may be spoken of as normal when taking place within not too crudely defined maxima and minima of the energy-frequency spectrum.

(B) Certain of these limits may be fixed by heredity, others by environment.

(C) Within these limits, however they may be fixed, there are relations of frequency-energy variability with "S," on the one hand, and "R," on the other.

(D) The electrocortical correlates of "S" and "R" are a measure of a part of the total flux of neurophysiological events that "are" psychological process.

The first two propositions deal with interindividual variability, while the last two deal with intraindividual variability. In this manner a definite link can be experimentally established to bind the two types of variability into a single conceptual framework. Still further, the determinants of interindividual variability can be treated in factual rather than in verbal terms, exactly as the determinants of intraindividual variability can now be treated.

The establishment of the neural limits of behavior in fairly strict terms will involve several investigatory patterns. At the outset, it must be possible to define behavior items which are presumed to enter into the relationship, and these definitions must be rigid. It must be possible to state the degree to which the exhibition of these behavior items is modifiable, in order that the causal

sequence of $\phi_{EEG}-$"R" or "S"$-\phi_{EEG}$ can be evaluated. It must be possible to state the degree of differentiation of individuals on the basis of the neural and behavioral scores not only as generalizations, but as they are actually exhibited—that is, there must be equal differentiation *during* the behavior that enters into the correlation.

Answers to these questions relative to the problems of personality and intelligence will either validate or invalidate the general conception of neural limits as applied to the normal range of behavior.

If validated, there will remain the question of the determination of the limits. Those investigators of constitutional bias will undoubtedly claim much for hereditary factors, while those of the opposite school will cleave to an environmental explanation. This does not seem to be a completely insoluble problem, however, if the basic assumption (basic to data herein evaluated) is accepted that there are a multiplicity of determining factors which are divisible under the two headings. One approach toward solution would be through the determination of the variation which can be environmentally induced, although this could not lead to information of a direct nature concerning the role of heredity. More direct evidence concerning the latter might appear by longitudinal studies. If it could be demonstrated that predictions of subsequent behavior variation were possible on the basis of nonenvironmentally determined EEG phenomena, constitutional factors could not easily be denied. The greatest difficulty involved in this problem is the lack of conceptual framework within which the terms "heredity" and "environment" can be manipulated.

However behavior may be limited, there still exists a wide area of research in discovering the relationships between intraindividual variability and "S" and "R," at least for the duration of our attempts to state the sequence of central processes contributing to the determination of "R." It is here that physiological psychology comes into the closest contact with nonphysiological psychology. If neural parallels of the postulates of such systems are obtained in *simple* behavior, the neural data of complex behavior become meaningful whenever the psychological systems erected on the simple embrace the complex. Such a reciprocal arrangement may be of assistance to the systematist; it will certainly be of assistance to the physiological psychologist.

Any attempt to place all such parallels in one physiological basket will probably lead to disappointment; the EEG may be

expected to contribute only to the extent that the processes so sampled are included in the organic processes involved in a given item of behavior. It is at this point that the arguments between the centralists and the peripheralists will be the greatest, but it should be obvious to both parties that the real task is to discover which neurophysiological indexes sample necessary factors and which sample sufficient factors for "R" and, perhaps of even greater importance, which sample factors neither necessary nor sufficient.

To the extent that the EEG can serve in the program of research devoted to the study of physiological determinants, or concomitants, of behavior, it is a technique of physiological psychology. In so far as it enables the experimentalist to sample determinants or concomitants which have heretofore been beyond the reach of instrumentation, it is a unique technique. The greatest dangers to its prestige will perhaps come not through the reaching of an impassable barrier but through the claims of its enthusiastic proponents. The EEG is one technique, sampling one type of activity; there are other activities involved in behavior, and they must be measured by other means. The grand total of the data secured by all techniques must be integrated to make ordering of physiological descriptions of behavior possible.

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A RÉSUMÉ OF MAJOR EXPERIMENTS IN ABSTRACT DESIGN ENDING WITH THE YEAR 1941

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The past 10 years have witnessed a quickening of interest in the psychology of aesthetics. From the technological viewpoint this has been encouraged by the growth of such statistical methods as factor analysis and by the improvement of numerous devices for the analysis of physical energies. From the social view there appears to be developing a heightened appreciation for the formal and the finished. This is particularly true of visual aesthetics, wherein the shape of an object has assumed commercial value. Increasingly, the public demands something more than the utilitarian in everything from motor cars to teaspoons.

The year 1941 is an opportune one in which to recapitulate. Research in art which was progressing in England may have been slowed to a military halt; a 10-year program at the University of Iowa has been completed; during this time there has come about a reordering of our theories of visual design. Consequently, it is with the hope of setting up some guidepost to the future that this résumé is presented now.

CLASSICAL EXPERIMENTATION

Work in visual aesthetics is old, relatively speaking. Fechner's studies of the aesthetic value of the mean proportional are well known as being among the first. They must be mentioned, if only because they constitute one of the earliest systematic attempts at verifying a theory in pure visual art (12, 13). Actually, though, the mathematical ratios of lines and areas do not prove to have great bearing upon the problems brought to light through experimentation.

In Fechner we find a tendency to idealize the mean. The next work of importance not only reflects a shift in aesthetic interest, but turns from a discussion of *the* man to empirical results. In 1894 Pierce, at Harvard, sought to validate the concept of balance by an experimental procedure (18). His subjects controlled the positions of regular shapes against a black background by means of pulleys. Measurements were reported of the mean distances of the

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several shapes from a given axis required to produce the experience of symmetry or balance in "0." The agreements of rank and direction among these data indicate that consistency existed among the subjects. These results are interesting because the decline of theories of innate aesthetic appreciation which were current in the prepsychological eras was favored by evidence of this sort. Here it is possible to relate the subject's hedonic experience to accustomed static relationships among physical objects. Thus, the subject's distaste for certain compositions might be attributed to an unanalyzed, but learned, awareness of violence to the architectural principles of statics.

Pierce's study concerns us because of the wide usage of the terms and principles with which he is dealing. He employs symmetry as a term to mean what the writer would describe as balance (7).[1] It is quite probable that the application of the two terms should make them synonymous.

The notions of Pierce were supplemented by those of Puffer, who introduced the idea of "vista" (19, 20). However, her work was more speculative than experimental. Legowski used empirical techniques to criticize the conclusions of both, but left the major problems unanswered (16). We shall return to the concepts of balance again for more thorough scrutiny.

Perhaps the major fault of these early experiments concerns the number of subjects employed. Pierce used six, including himself. Since he reported strong individual differences, and consequently dealt with statistical averages, he would have strengthened his work greatly by studying a larger population of judgments.

We have seen that aesthetic judgment was thought to depend partly upon the adherence of the composition to the laws of statics and kinetics. It was but a short step to the incorporation of subjective feelings of stability and movement suggested by the parts of the picture. Thus arose the empathy theory of design (7, 24). It was not hard to elaborate this concept into the simulation of human bodily movements. Thus it was conceived that a pictorial composition would seem restful if its elements consisted of long, continuous lines, especially if these lines were not to intersect in numbers, but were arrayed horizontally or vertically.

It seems necessary to cross a quarter of a century in order to introduce the next important investigation which might logically

[1] "Symmetry refers to the exact correspondence of two or more parts of a design; balance refers to a more subtle relation of parts which gives them equal 'weight' (whatever that may turn out to be) without close similarity" (7, p. 51).

be grouped with the ones already discussed. This is the work of Helge Lundholm, and it may well be classified as belonging to the empathy heritage (17). He sought to test the notion that certain types of lines correspond to such adjectives as sad, quiet, lazy, merry, agitating, and the like. Altogether he used a total of 45 words with synonyms, which he divided into 13 groups. His eight subjects were required to draw lines suggested by the list.

The data now consisted of a number of lines drawn in response to the suggestions contained in the list of words. These he attempted to categorize into three divisions: lines with only curves, lines with only angles, and lines with both curves and angles. The two major groups of curved and angular segments were further subdivided into three orders each. These three categories were determined by the degree of frequency in alternation of direction, equivalent to the words: curved, wavy, and rippling.

With data made up of a neat classification of drawn lines, he then divided them into percentage groups to show that certain classifications of lines correspond to particular groups of adjectives. The results show statistical agreements which far exceed chance, but unfortunately there appear too many unforeseen criticisms to permit extended applications. In the first place, one finds it hard to accept the assumption that the act of drawing a line is the inverse of having a specific feeling or emotion on seeing the same line in a composition. One finds it difficult to accept the supposition that eight subjects are representative, perhaps even of individuals from widely differing cultural backgrounds. Finally, the percentage treatment appears to be cavalier. It introduces an element of the experimenter's subjective decision concerning the category in which a drawn line should be included.

THE IOWA EXPLORATIONS

In attempting to sketch the course of experimentation in visual art the writer may be guilty of arbitrary classifications, which quite possibly the original experimenters themselves would reject. When he declares that the Iowa studies in children's sensitivity to balance, rhythm, and unity belong to the grouping of the empathy theory, it is only because these words have been closely associated with the term "empathy." Regardless of origins and theoretical implications for psychology, artists have incorporated "balance," "rhythm," and "unity" into their own theories and lessons.

The Iowa studies may be treated as a whole, since the numer-

ous investigations have in common the presupposition of the validity of the concepts tested. In the first group of experiments to be published little effort was made to validate the criteria by which children's performances were judged. Consequently, work with children in the State University nursery school involved the perception of "balance" by the act of piling blocks and of reproducing a given block arrangement, of "rhythm" by filling in a gap in a series of cut-outs with a figure which reproduced the series (both pictorial and abstract), and of "unity" by allowing children to arrange a formal garden group.

The first two of these studies produced reliable positive results; the last, which admittedly did not, is valuable because it contained a preliminary validation study. This preliminary validation demonstrated agreement between architects with respect to rank judgments on "balance." From the start of the Iowa studies there was an attempt to attack the problem ontogenetically; and it is unquestionably true that those investigations which are reliable at all tend to show progressive improvement in scores through successive age levels.

While these studies are undoubtedly original pieces of pioneering, it is questionable that they are wholly valid. The balance investigation is open to the criticism that the experimenter fails to report a criterion of balance which can be reproduced by other and subsequent students. In addition to this, the limits of tolerance employed in the scoring procedure appear to be purely subjective.

The rhythm study is vulnerable to the more serious objection that the task might very easily have been solved as a matching act with no aesthetic value involved. Furthermore, a child's response that he "likes" one substitution better than another may, or may not, be analogous to the same statement from an adult. Perhaps the study was performed with the understanding that, although the two are not strictly comparable, the former is the raw stuff from which the latter develops. The question is a complicated one which wants more investigation. It is certainly not surprising to the writer that a two-year-old child can discriminate between a rabbit which is sitting up and one which is running. All in all, the studies did succeed in demonstrating that children exhibit a gradually maturing sense of fitness in placing blocks, toy trees, and cut-out patterns. If we accept the argument that aesthetic pleasure is derived from such conformities, then we run the risk of accepting a circular form of deduction—assuming that people pre-

fer balanced designs, they will choose balanced designs; since children selected balanced designs, they must have preferred them. Q.E.D.; we have shown that children prefer balanced designs.

There are many more of the Iowa studies in visual art. The writer regrets that he cannot do justice to them with the space and comment which such an ambitious program warrants. Those portions of the three volumes which are concerned with aesthetic aptitudes in any of their forms are outside the scope of this summary, while the articles which deal with aesthetic theory alone must await a discussion which is not primarily experimental. Nevertheless, we shall be well rewarded by examining several more of the Iowa researches.

In Walton's investigation of children's empathic responses (24) we find an attempt to bring the word-line technique developed by Lundholm to the level of a kindergarten child. Through his preliminary studies Walton seems to have obviated many of the objections to Lundholm's work. While it is yet too early in experimental chronology to say that an empathic basis for line has been established, still one is impressed by many of Walton's observations. This may be said despite the unreliability of his tests when applied to the problem in which he used them. One of his most significant conclusions states that "the child tends to think in terms of pleasantness-unpleasantness." Thus, if Walton's generalization is correct, hedonism and empathy are one from a very early age. We may summarize by saying that while empathy plays a role, its importance is an unknown quantity.

So far we have conceived of art principles as empathic experiences. The transition from this conception to the theory that they depend upon innate functions of the nervous system can be made by reference to Kellett's study of aesthetic unity (15).

If one adheres to an empathic view, his orientation is environmental; if he takes the Gestalt stand, his interest reduces itself eventually to the neurological, contradictory as this may seem. While extreme protagonists of either viewpoint are rare, still some lean more toward the one than the other.

Kellett endeavored to show that persons of high school age prefer the "unified" pictures made by artists to the "poorer Gestalten" of photographic "reality." Her method was the technique of choice between art works paired with their photographic "counterparts." In this she was unsuccessful, seemingly invalidating the Gestalt view. What she actually showed was that in experimentation on aesthetics (as in any other form of study) the influence of

an uncontrolled variable may be so great that it completely nullifies the effect of the independent variable under test. Notwithstanding all this, her results have a comparative value which should be given full consideration.

In general, the photographs were preferred to the paintings. The reason for this is largely speculative, but a possible explanation is the greater similarity of the photographs to perceptual "reality" (whatever that is). Without championing the Gestalt position, the writer agrees with Miss Kellett that "research is needed on *experiential unity* in addition to *objective unity*," but he hesitates to say that it will ever be obtained by the techniques used.[2] Had photographs been paired with photographs and paintings with paintings, the results might have been different.

THE BACKGROUND OF THE NEUROLOGICAL CONCEPTION

The Kellett study may be our introduction to the innate view; but in order to understand this theory in its entirety we must return to an earlier period in the history of aesthetics. The notion of innate functions, like much art theory, is also old. Many of its past experiments lend credence to the Gestalt conception of aesthetics. Moreover, recent evidence tends to support the theory of the aesthetic value of unity. Of course, unity may be a basic concept of either the neural or the empathic theory; but one is inclined to associate such terms as "closure" and "Gestalt" with the word unity. Certainly there are differing connotations attached to it by the two viewpoints. One conception regards it as the summation of empathic experiences, while the Gestaltist looks upon the quality of unity as being irreducible. Such an irreducible quality must lie in the human nervous system, for here alone is a pattern which cannot be altered at will.

On the other hand, it has been the fashion for art theorists to attach the attributes of aesthetic value to the observed designs. If theories are to exist and to be tested, it is necessary to correlate the response with the stimulus situation. Thus, the picture itself represents the stimulus situation, but the "laws" of organizing the observed design are thought of as being part of the individual. Accordingly, a person confronted by some object of art perceives it, or fails to, because the elements of the pattern are organized by the activity of his nervous system. Consequently, persons see alike because their nervous systems work alike; but one perceives the same as another only if the stimulus object remains constant.

[2] Kellett (15), pp. 49–50; the italics are Miss Kellett's.

It is not surprising to learn that "laws" which define these operations of the nervous system have been attempted. Probably the most daring of these is the Birkhoff formula. It is assumed to extend to poetry and music as well as to visual pattern. Since it is the ratio of the organizing factors to the complexity of a design, measured upon a partly objective basis, the formula is readily amenable to experimental testing. Birkhoff constructed a series of forms, mainly regular, two-dimensional shapes, to which the "aesthetic measure" might be applied (2). The evaluations which Davis obtained experimentally for these forms have been referred to rather frequently in later experiments designed to test the validity of the Birkhoff hypothesis (9).

After preliminary testing, the formula was altered to encompass new variables. Beebe-Center was among the first to subject it to experimentation using the Birkhoff figures (1). His paired comparisons were interpreted as lending limited support to the "aesthetic measure." Subsequently, Brighouse performed a similar study using 18 of the Birkhoff forms and having them paired by 22 adult subjects (4). The correlation between the "measures" and the paired-comparison scores was +.77, which was "taken to indicate a satisfactory degree of reliability." Brighouse reported that this portion of his experiment was preliminary to testing an hypothesis by C. O. Weber and his predecessors—"essentially the Fechner *unity-in-variety* principle." That Brighouse failed to accomplish this end with a widely assorted group of 352 subjects is partly due to the supposition that the Birkhoff formula is an aesthetic measure—in the exact sense of the word "measure."

Wilson performed his own paired-comparison test of the Birkhoff formula, using Birkhoff figures, and concluded that in visual art the Aesthetic Measure is practically worthless (26). Finally, Harsh, working with the Beebe-Centers, sought to discover just what variables are involved in ranking the Birkhoff forms (14). After having 20 designs ranked he was able to assign a score to each figure and to correlate the placements by pairs of judges. The experimenters factor analyzed this matrix of person-to-person correlations and finished with four factors. The somewhat subjective interpretation of these centroid factors was that they stood for persons who liked: (1) smooth contours; (2) simplicity; (3) symmetry; (4) odd points, or tall tapering figures. Just what these factors mean, and just how valid they are, one is not in a position to say at this writing.

The Search for Aesthetic Factors

The aforementioned factor analysis by persons is in itself significant. If there exist sharply defined classifications of individuals, the Gestalt theory must be greatly modified. This follows because the Gestalt theory implies that the neural bases of cortical function in perception are essentially the same for different people.

The Harsh analysis is but one of the many attempts at a person-to-person factorization of aesthetic evaluations. Others have been made by Burt (5), Stephenson (23), Dewar (10), Davies (8), and Eysenck (11). Miss Davies' study approaches the heroic by factorizing all the available tables of affective judgments, many of them still in the files of the Harvard and Cambridge psychological laboratories.

Even more recently than Miss Davies, Eysenck has employed factorization to approach Birkhoff's problem (11). He has evolved an aesthetic equation by summating factors obtained from the analysis of correlations between the ranks of Birkhoff polygons. The data were taken on two sets of 32 of the figures given in the *Aesthetic measure.*

Eysenck's empirical formula consists of a series of factor weights, each proportional to the amount of the variance for which the factor accounts. An attempt was made at supplying a qualifying description of each factor and at further identifying it by statistical methods. The whole formula was put to a new test by retaining 10 of the first designs used and adding 5 more. After obtaining ranks for this second set of figures, the equation was applied to determine its power to account for the intercorrelations between their values. The additional rankings were made by a new set of judges. Altogether, it is stated that the formula accounted for 90% of the variance in the first experiment, but for considerably less in the second. He presents a certain amount of support for the contention that the formula accounts for about all the agreement in the second experiment which is open to test under the circumstances.

One assumes from the general nature of the experiment that the first two matrices were composed of correlations between 14 persons. Thus the factors are personal ones—removed according to the precepts of the Psychological Laboratory, University College, London. Consequently these should be contrasted to: (1) test factors, obtained from correlating *tests* (or design ranks), not individuals; (2) personal factors removed by a modification of the

Thurstone Centroid Method, as used by Harsh and the Beebe-Centers.

At this point no one is in a position to say which factorizing procedure is the more justifiable. It might be suggested that both sets of data be submitted to the same techniques. Should different factors result, then it may prove necessary to rework older tables of aesthetic correlations.

From the available factor studies, both aesthetic and otherwise, one suspects that a great deal of time has been lost in carrying the arithmetic of factorization beyond the limits of the reliabilities of the data. More attention to controls should be put into the original experiments. From unpublished data which have recently come into the hands of the writer he is convinced that tests of factors past the second and third can be made more crucially than has been done. Many studies have been performed on matrices, the highest correlations of which were not more than .50, either plus or minus. Of course such matrices may yield statistically reliable factors, but on the surface it would seem that the chances of significance for factors obtained successively from them would be far less than from a matrix which ranged from $-.50$ to $+.85$. In an experiment now in progress at DePauw University aesthetic ratings of pictures have already yielded personal reliabilities ranging from $+.35$ to $+.996$ and averaging well over $+.70$.

The severest criticism which can be raised against most of the present studies is that retest reliabilities have been neglected. This results in a fundamental vagueness concerning the standard errors of individual rankings. It is not sufficient to establish the reliability of a ranking by giving its sigma obtained from the various subjects' judgments on a single trial. In other words, the high correlations just mentioned are still inadequate. This is because the scores used in correlating persons depend upon the stability of each separate subject's evaluation of each separate design. The degree of that stability cannot be inferred from the spread of other subjects, nor even from his own spread upon a single trial. The only way to obtain the standard error of an individual's judgment is by testing him again. Until this is done, it would seem as though we cannot have any real criterion of the reliability of a residual. Looked at from this viewpoint, the limited agreements which do exist between the available experiments are heartening rather than discouraging.

The disagreement over which factor technique should be employed tends at the moment to produce but slight confusion in

experimental results. Nevertheless, the conflicting conclusions will multiply in proportion to the number of new studies.

In a summary such as this it is impossible to give a detailed account of the theories involved in the various techniques. However, the main disagreement arises from the weighting of the first factor. This is generally maximized by the British factorists when attempting to account for the variance of a correlation matrix. It has been claimed that the "bipolarity" frequently met in the second factor is the result of emphasis upon the first. This may not be altogether true, since the Thurstone method is to cut the Gordian knot by a reversal of signs. Anyway, an interpretation of a "bipolar" factor derived from correlating persons is to say that certain individuals are diametrically opposed to each other with respect to this factor. Since the Thurstone technique developed around assumptions about the intercorrelations of *tests*, there may still be room to rationalize the two views.

The suppositions underlying the centroid method do deny the existence of negative traits. Accordingly, the factor matrix is "rotated" until the significant loadings are positive, regardless of the effect of the rotations upon the magnitude of the first factor loadings. Since either technique is statistically justifiable if the proper psychological assumptions are made, it is impossible to settle the issue upon purely theoretical grounds. It is one of those things for which an immediate answer does not seem to exist.

This methodological controversy is more serious than appears on the surface. Eysenck, paralleling the Spearman two-factor concept, has postulated a general factor of aesthetic appreciation (11). He does this with the support of the Davies investigation mentioned above (8). In doing so he seems to express certain Gestalt assumptions. If his suppositions concern the basic neurology of types, as they should to be theoretically consistent with Gestalt psychology, there may be evidence to invalidate his beliefs.

One such bit of evidence is the fact that the writer has found that the ability to perceive "balance," "rhythm," and "unity" is susceptible to training. Indeed, this ability is so susceptible that mere suggestion that any one of these three qualities be sought out in examining an abstract design is sufficient to greatly alter the affective tone of the figure under inspection. This, despite the fact that the subject may be untrained in art and ignorant of any formal definition of the terms.

To discover the effect of training to perceive balance, symmetry, rhythm, and unity the writer caused 10 subjects to rate 50

abstract designs for "pleasantness" (25). Then, during a six-week
program of experimentation these judges learned to estimate de-
grees of balance, then symmetry, then rhythm, then unity in the
same designs. After rating the figures for these qualities the sub-
jects followed each perceptual rating by ratings for pleasantness.
Correlations were run between ratings for pleasantness and estima-
tions of the degree of each quality. Correlations were also obtained
between the quality ratings and similar ones by a group of five
artists. For the 10 experimental subjects the correlations between
pleasantness and design quality rose in every instance for the prop-
erties of balance, rhythm, and unity. Furthermore, after training
the 10 subjects agreed with the pooled artists' ratings for each of
these three qualities to the extent of correlations which exceeded
+.75, or well within the reliability of the ratings by either group.
Nor can the explanation for this sensitivity to specific training be
attributed to cumulative or transfer effects, as the group was
carefully sectioned on different days and in different training or-
ders to prevent this.

The writer does not presume to deny the probability that
there exists in the population a strong common factor which may
be tentatively called an aesthetic one. Nor is the foregoing dis-
cussion intended to disavow the existence of group and individual
factors in artistic perception. Rather, it is offered as a caution
against the type of interpretation which can be put upon a general
factor. There are a number of possibilities which are being enter-
tained in modern theory. One of these, to be taken up presently, is
the neurological approach to perceptual pattern. Another is the
community of empathic experiences. Still another, which in ab-
stract design is less important, is the community of specific ideas
and associations which can be found in any given society.

IMPLICATIONS OF THE FACTOR CONTROVERSY

Another experiment which has been carried on during the past
two years by the writer may add light to the problem of factorizing
tables of intercorrelations between aesthetic ratings. In this, cor-
relations were computed between tests.

At this writing the only other existing application of the inter-
test method is Miss Dewar's larger study of four art judgment tests
as taken by girls. The problem of the relationship between the two
forms of factor analysis is closely associated with the question of
the relative influence of empathy and innate function upon hedonic

experience. It can be argued that if the tests can be classified into various categories, while individuals cannot, then it must be presumed that persons function alike. This would lend support to the theory of innate preferences, though by no means proving it.

If, on the other hand, the classification of individuals proves to be valid, the source of their divisions will still remain unknown—variation either in training or nature may be responsible. Finally, it is quite probable that an even more complex condition exists; that all forms of variation are present to a considerable extent. If this is the case, as revealed by factor analyses, factors pertaining to clusters of persons may tend to maximize whatever variables differentiate them, with a resulting emphasis upon the similarities among tests rather than their differentiating features. Conversely, factor analyses of intertest correlations may tend to maximize all those variables which discriminate the designs. Should it develop that the two approaches merely emphasize different aspects of the same problem, the student of experimental aesthetics may find himself in possession of an invaluable tool. It may prove to be a technique by which he can study the relative effects of training and nature upon hedonism.

MATHEMATICAL BIOPHYSICS

Up to this point attacks upon the question of formal factors in art have been psychological, but now the theories of mathematical biophysics are being extended to many of the aesthetician's unanswerable problems. Factors derived from analyses of tables of intercorrelations, theoretical variables within formulas of "aesthetic measure," even statistical means, always leave us with unknowns which cannot be reduced or "explained" in the causal sense of the word. Eventually the psychologist must reach what he terms an "element," whether it be an elementary Gestalt or sensation. The upward elaboration of mathematical biophysics into realms of increasing complexity has suggested to Rashevsky a theory with which he hopes to account for the formal aspects of visual aesthetics (21, 22).

At present the published equations are limited to figures bounded by straight lines. He has used the weights obtained by Davis for 10 of the Birkhoff forms (9). Against these he has plotted hedonic scores determined deductively from a set of mathematical assumptions about the neural activity of persons while observing a pattern. The result is a near congruence of the 10 points.

Whether extensions of this theory now being attempted will account for the aesthetic effects of curvilinear shapes remains to be seen. At least the investigation is now under way, both deductively and empirically.

Another interesting mode of approach involves a study of the observing act itself. This becomes possible if a single subject be used at a time. By displaying a picture tachistoscopically, Brighouse was able to count the fixations of an individual as he observed a painting. He then compared the number of fixations and their positions for all his subjects (3). He found differences between those trained in art and those technically naïve. Certain paintings which might be classified as "obvious," either in composition (formal structure) or in meaning, readily lost their interest for the art group. Attentive fixations were numerous for the more subtle or involved works in the case of the art subjects. Furthermore, the general course of successive fixation positions was about the same from observer to observer. Interestingly, this course did not follow precisely the major contours of the painting, but was rather more inclined to "jump" from one point of attention to another. Since Rashevsky has theorized that a hypothetical "following" of the outline of a form is responsible for the degree of pleasantness it will have, we can see that the works of Brighouse and Rashevsky may have some complementary features. Thus, training in formal art may consist in the increase in variety and complexity of patterns which an individual can "follow" or perceptually organize. Consequently, a trained artist may experience "lawfulness" in a picture which is completely incomprehensible to the neophyte. Not only would the proof of such a concept tend to explain the positive results obtained in several experiments on aesthetic unity, but it would aid greatly in understanding many nonaesthetic forms of perceptual behavior.

GENERAL AGREEMENTS

In concluding this evaluation of some of the major work which has been done in formal visual aesthetics, it may be well to describe the entire progress by referring to one more study. Very little has been said of the reliability of the various aesthetic evaluations cited. In particular, nothing at all has been remarked concerning the temporal consistency of such scores. This consideration, however, must be the first interest of any investigator in psychology. Otherwise the development of mathematical functions

would be a waste of time. We have in Mrs. Cahalan's study of the consistency of aesthetic judgment a survey of preferences on 63 pairs of pictures made with high school and college art students after a lapse of one year (6). The writer takes the liberty of quoting her entire conclusions:

Within the limits of this study the following conclusions were drawn:
1. In general, aesthetic judgment is consistent within groups, and in individuals over a period of a year.
2. Art students are more consistent than non-art students, both as individuals and as a group.
3. High aesthetic judgment scores tend to be more consistent upon repetition of the test than low scores.
4. Age did not seem to be a significant factor of consistency in the group studied.
5. Girls untrained in art are slightly more consistent than boys untrained in art. This sex difference does not appear among art students.
6. Consistency of response depends upon materials used, as well as upon the observer.
7. Aesthetic judgment is positively related to conscious knowledge of compositional principles (6, p. 87).

The writer quotes these interpretations because he has found several of the consistencies in his own data. While these data were secured through ratings of abstract designs, and are not strictly comparable to Mrs. Cahalan's in either material or treatment, yet the essential agreements are pronounced. More agreements of this sort will give to the psychology of aesthetics a unique position among the specialized branches of the major field.

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BOOK REVIEWS

PORTEUS, S. D., with the assistance of M. Hunter & C. J. Herrick. The practice of clinical psychology. New York: American Book, 1941. Pp. viii +579.

The author disclaims any attempt to cover the entire field of clinical psychology in this book. His purpose is technological rather than theoretical in that he seeks to "epitomize his own actual practice in clinical psychology." For more than a quarter of a century Dr. Porteus has held a frontier post in the field of clinical psychology. As director of the Psychological Clinic of the University of Hawaii in Honolulu his everyday work has brought him in contact with a greater variety of clinical material than crosses the threshold of any other clinic in the world. As professor of clinical psychology he has directed the training and research of a considerable number of graduate students. He himself has done notable research in the field of racial psychology, first with the Australian aboriginals and later with the Bushmen and other South African groups, as well as with the dozen or more racial groups at his hand in Honolulu. The reader will be disappointed to find that the author takes his own statement of purpose so literally that the book deals almost exclusively with aspects of clinical work that would be found in any psychological clinic. For example, the author is in a position to write an outstanding chapter on the advantages of clinical studies as a research method. Or he might well have discussed more fully the training that, in the light of his experience, is desirable for the clinical psychologist.

The discussion of clinical problems and methods (Chap. II) shows what may be expected of the clinical psychologist in connection with the cases brought to his attention. But the author seems overcautious or overmodest in his statement of how much the psychologist should undertake to do in regard to these problems. "He will function in a diagnostic and consultative capacity rather than in a directive one. He makes no claim that his special training gives him the right to dominate the social-adjustment situation." Why speak of a "guidance" clinic at all if the work is not to be "directive"? It may be better technique to get a parent to come to the conclusion that he is too strict with his child and to let him feel that trying less severe methods is his own idea than to dictate this change in treatment, but who will say that the indirect method is not directive? Or why be overcautious in a discussion of the role of the psychologist *vs.* that of the psychiatrist? Why should this question be answered categorically? In a given situation the answer should be a practical one. Given a certain problem, whether of analysis, diagnosis, or treatment, and the question of whether a particular psychologist or a particular psychiatrist should deal with the case, the answer should depend upon the relative merits of the training and the professional competence of the two men and not on their designations.

The author is at his best in his discussion of the use of psychometric techniques and their interpretation and in the application of such methods

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to the study of various groups. Here he is on more certain ground and makes no apology for the role of the psychologist. These chapters give the book a substantial significance. The specious simplicity of psychometric scores and careless misinterpretation of them have tended to discredit psychology as well as to retard the development of thorough methods in clinical examination. The author's very sane treatment of the diagnosis of mental deficiency, for example, should be read carefully. It may not of itself eradicate the idea that persists even among psychologists that a Stanford-Binet IQ of less than 70 is of itself diagnostic of mental deficiency, but it should have a wholesome influence in this direction.

Dr. Colin J. Herrick, one of the author's associates, contributes an excellent chapter on the diagnosis of reading disabilities. He might well have supplemented the rather meager treatment of "reading readiness" appearing earlier in the book. The school psychologist should play his part in the prevention of reading difficulty as well as in its diagnosis and remediation.

The author's closing chapter on clinical service and relationships contains his philosophy of the human relationships involved in clinical practice. He is relatively conservative, is frankly skeptical about "freedom in self-expression" and other catch words used by the proponents of progressive education, and feels that sincerity and common sense may be most helpful when no attempt is made to dress them up in scientific jargon. Even if child training is not repressive, there must be some measure of "social ritualization." "Good living must be learned; it is not entirely self-taught."

The critical instructor who uses this book as a college text will find different parts of decidedly uneven merit. This may be an advantage in that his students will get training in critical evaluation and will find excellent material for class discussion. In any case they will have a genuine sense of their indebtedness to the author for sharing with them his long experience in the practice of clinical psychology.

FRANCIS N. MAXFIELD.

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GREENE, E. B. Measurements of human behavior. New York: Odyssey Press, 1941. Pp. xxi +777.

The expressed intention of the author was to prepare a book in which behavioral measurement techniques and procedures would be treated more comprehensively than they are in available volumes. It is a credit to Professor Greene that such an ambitious project has been realized in a manner that will please the three most interested groups: instructors who are seeking a textbook suitable for the more modern course in measurement; students who have expressed (at least to this writer) their despair with texts which from preface to bibliography credit them with too much in the way of preparation; and clinicians who have long awaited a convenient and descriptive practical guide.

In contrast to the majority of volumes, in this work the application of measurement principles and procedures beyond the field of intelligence *per se* is discussed with commendable restraint. The author recognizes

that, while measurement has been extended into many new fields, we have not yet come far enough to make positive claims with unmodulated voices. The result is a work that covers the field of measurement in a critically objective fashion, resorting for the most part to exposition based on primary sources carefully evaluated.

The basic considerations of measurement form the material for the first seven chapters. Here are discussed the nature and types of measurement, statistical tools and facts employed in the construction and utilization of measuring instruments, and factorial analyses. Adequately prepared students should have little trouble with this section, although the present writer believes the chapter on factorial analyses is the weakest part of the book. Measuring instruments and results occupy the mid-section; and here are to be found the major types of intelligence scales, a chapter on achievement testing, chapters on group testing, performance, mechanical and motor tests, measurement in the arts, academic and vocational interest tests, appraisals of attitudes and public opinion. Also included are three chapters under the rubric, "Modes of Adjustment." Here free-association scales and inventories, conduct measures, observations, and laboratory methods are considered. The section closes with a chapter on the evaluation of methods and results in this branch of measurement.

Persistent problems occupy the final section. In this we find headings such as: effects of practice on scores, measurement of growth and senescence, absolute scaling, evaluation of judgments, and measurement of native differences.

The volume is liberally supplied with illustrative material, graphs, charts, and tables. For a refreshing change these appear for the most part on the same pages with expository matter relating to them, thus obviating bothersome turning of pages to find the graphic referrals mentioned in the text. On the whole, the format reflects the excellent coöperation of author and publisher, resulting in a book of pleasant, yet functional, aspect.

ROBERT M. LINDNER.

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VALENTINE, W. L. Experimental foundations of general psychology. (Rev. ed.) New York: Farrar & Rinehart, 1941. Pp. xvi +432.

This is a revision of the well-known volume of readings of the same name first published in 1938. The general plan and purpose of the book remain the same. The principal changes consist in a rearrangement of topics, the omission of the chapter on "Market Research," and the addition of two chapters, one on "Conditioning" and the other a summary chapter at the end of the book. Other changes in content include a new section on "Extra-Sensory Perception" in Chapter I ("The Nature of the Psychological Experiment"), sections on "Grasping and Creeping" and "Hopi Children" in Chapter VI ("Maturation and Growth"), and the topics of "City Life" and "Primitive People" in Chapter VII ("The Meaning of Intelligence Test Scores"). In Chapter VIII ("Physiological Conditions") a section dealing with the effects of high altitudes has been

added. The former chapter on "Drives and Motives" has been divided into two parts; the first retains the original title, and the other is called "Motives and Incentives." In the former, the sections on "Changes in Incentives" and "Substitute Incentives" are new. The chapter formerly called "Conflict" is now entitled "Frustration." The order of topics in the chapter on "Suggestibility and Hypnosis" has been changed, but the contents remain the same.

One of the most noticeable changes, in addition to those already mentioned, is the inclusion of a good deal of interpretative comment. This book was originally intended to consist of a collection of representative experimental studies to parallel and supplement the regular textbook in general psychology. In prospect, it seems a simple task to select representative studies from the experimental literature and, by a simple process of condensation, to summarize and present the essential facts of the experiments and then leave these facts to speak for themselves. In such a way the student should become familiar with the devices, techniques, and procedures used in psychological investigations. From such a sampling of actual psychological investigations there should arise a much more adequate conception of what psychology and its methods are than is ordinarily derived from generalized descriptions of psychological procedures and abstract definitions of its subject matter in the traditional manner.

However, in actually working out such a program, it is discovered that few experiments lend themselves to such condensation and self-interpretation. The significance and implications of the average experiment are not at all obvious to the uninitiated. So it becomes necessary to add interpretative comments and additional explanatory matter in order to give the results meaning. As the interpretative and explanatory portions increase in amount we approach the conventional textbook instead of a supplementary volume of condensed experimental studies. The reviewer has the feeling that that is what is happening to the present volume. There is certainly some loss in thus increasing the explanatory and interpretative portions of the book at the expense of the experimental and factual. There is considerable likelihood that it will become just another text. The writer still has the conviction that there is much value to be derived from an objective presentation of the methods and results of experiments and then leaving them to speak for themselves.

On the whole, the studies are well chosen, the selection and condensation of pertinent data are effectively done, and student interest should be easily maintained throughout the volume.

C. W. TELFORD.

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SEASHORE, C. E. Why we love music. Philadelphia: Ditson, 1941. Pp. vi + 82.

On many previous occasions Dean Seashore has told the psychological world of his important musical researches. Music teachers have been kept informed through the medium of the *Music Educators Journal.* Now parents are having their turn with a book which should also interest the

aesthetician, the music educator, and the psychologist. *Why we love music* is a collection of five previously published essays, with an additional two chapters which round out the area of interest. The author was motivated to write the volume by a gibe from *Time* in which psychologists were taken to task for not explaining why we love music.

Seashore's latest book goes beyond the findings of the laboratory. Much of it is in the nature of personal conviction, broad generalizations, and predictions with which the author anticipates and, in fact, welcomes disagreement if it will lead to further investigation and practical trial. Each chapter closes with a "Thought Review,"—a list of general principles, questions to consider, and true situations to discuss. The language throughout is simple and clear, and the style is interesting. The chapters cover considerations of the reasons for musical appreciation, of the proper sorts of music for persons young and older, of musical temperament, inheritance, the future of musical instruments, and a final chapter in which the progress of music up to this date is assessed.

Dean Seashore is to be congratulated for lending his support to the now growing philosophy which regards music as having pleasurable possibilities for all, not only for the favored few. Too long have music teachers regarded their task as that of creating either virtuosos or music teachers out of their often rather unpromising material. In keeping with this philosophy, Seashore holds that the preschool child should be surrounded by beautiful music and that the child from six to ten should have music lessons in school along with his work in the three R's. But, except in unusual cases, private lessons had better wait until age ten, when the child's aptitudes and interests can be more easily studied.

The author's hereditarian biases are shown in his statement that "the musical temperament generally has an hereditary basis in a highly-strung nervous disposition" (p. 48) and in the reiteration of his stand concerning the "elemental capacities" his measures tap. With this extreme position the reviewer has disagreed on other occasions.

The publishing of *Why we love music* through a music publishing house was a wise move, as it assures a musical audience. It is to be hoped that members of parent-teacher groups and discussion clubs will also note this tiny book, as they will be amply repaid for the few hours the 82 pages of reading will cost them.

PAUL R. FARNSWORTH.

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